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V.—THE AERODYNAMIC RESISTANCE OF SPHERES, SHOT UPWARD TO MEASURE THE WIND.

By Lewis F. Richardson, *F.Inst.P., F.R.Met.Soc.*

Received September 14, 1923.

Abstract.

The resistance of the air to accurate steel spheres has been measured by two new methods (1) by observing the tilt from the vertical at which the sphere must be projected in order that a measured wind-distribution may bring it back to the gun ; and (2) under circumstances similar to those just described by comparing the muzzle velocity with the time of flight of the sphere. Measurements of the resistance of varous spheres by about twelve different authors are collected in a single diagram, and the remarkable divergences between them are discussed, in relation to turbulence of the air, rotation of the sphere, and deviations from sphericity.

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§1. Introduction.
§2. Comparisons with the wind, measured by pilot balloons.
§3. Constants determined from time-of-absence and muzzle-speed.
§4. Summary and conclusion.
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§ 1. Introduction.

THE pressures exerted by a fluid on a sphere moving through it have not yet been correctly deduced in general from mechanical principles. We must rely mainly on experiment combined with a little theoretical guidance. Perhaps the two chief theoretical advances have been, firstly, the deduction by Stokes of a law applicable in restricted circumstances, which, however, lie outside the scope of this Paper, and, secondly, the introduction of compound variables of zero dimensions, a procedure suggested by the late Lord Rayleigh and applied to spheres for the first time by Bairstow.(10)

Following Bairstow,(10) let us discuss the results by the aid of a diagram (Fig. 1) which has for co-ordinates φ_1 and φ_2, where

$$\varphi_1 = \frac{\text{Force of resistance}}{(\text{density of fluid})\ (\text{speed})^2\ (\text{diameter})^2}$$

$$\varphi_2 = \frac{(\text{speed})\ (\text{diameter})\ (\text{density of fluid})}{(\text{viscosity of fluid})}$$

These variables φ_1, φ_2 are pure numbers, when any self-consistent set of dynamical units is employed. It would have been better if we could have made a solid model having a third co-ordinate φ_3, where

$$\varphi_3=\frac{\text{speed of sphere relative to air}}{\text{speed of sound}}$$

but, in lieu of this, φ_3 is marked on the diagram if it exceeds 0·7.

When Stanton and Pannell represented their own experiments on the flow of liquids in pipes by plotting φ_1 against φ_2 it was found that the observations lay on a single curve, in spite of the fact that various sizes of pipe, various speeds and two fluids were used. We might accordingly expect a single curve on the φ_1, φ_2 diagram to represent all the observations on spheres. How different is the actual diagram may be seen from Fig. 1.

Bairstow's(10) line is shown in the lower part of the diagram. It is derived from Eiffel's(11) experiments in wind channels and Shakespeare's(12) on falling celluloid balls.

Pannell(4) has collected and discussed the results from seven laboratories. In all but one set there is a critical value of φ_2, near to which an increase of φ_2 is accompanied by a very rapid decrease of φ_1; but the numerical value of the critical φ_2 is different for different laboratories, and ranges from $0{\cdot}6\times10^{+5}$ to $2{\cdot}6\times10^{+5}$. Pannell considers that this critical value of φ_2 is greater or less according as there is little or much turbulence in the fluid approaching the sphere. As an instance, Pannell contrasts the non-turbulent water in which Costanzi towed his sphere with the turbulent currents of experimental air channels. (*See* Fig. 1.)

Brazier(13) on the other hand, has reduced observations made either in the open air, or in still air, on indiarubber pilot balloons. He also makes the suggestion that in a certain critical range $1<10^{-5}\varphi_2<3$ the resistance in a wind-channel depends largely on the turbulence of the current; so that open-air results may well be different. I find that our φ_1 is 6·29 times Brazier's constant k, and I have simply converted his unsmoothed numbers, plotted them on Fig. 3, and drawn a line through them. Recently Horiguti(17) has published the results of experiments on balloons, with threads attached, rising in still air. Our $\varphi_1=3{,}600\times981\times4/\pi$ times Horiguti's k. His observation gives the point marked V. at the upper extreme of the series of balloon data.

F. S. Schmidt(16) observed wax spheres falling freely in water, and found $\varphi_1=0{\cdot}17_3$ when $1{,}200<\varphi_2<3{,}000$ for constant velocities. The corresponding mark is at the extreme left of our Fig. 1. For accelerated motion, Schmidt found a more complicated law of force.

Of experimenters using the accurate steel spheres which are made for ball-bearings, there have been H. S. Allen(14), who dropped them in water, and R. G. Lunnon(9), who dropped them in air down coal-mines. Our φ_1 is $g/4$ times the k of Allen's table VIII., from which I extract the data. Mr. Lunnon kindly allows me to show his result, although it is not yet published.

Steel spheres made for ball-bearings have been used by the present writer for the measurement of upper wind. Having been shot upwards in a direction slightly inclined from the vertical, the sphere in falling reaches the ground near to the gun. A full account of the practical manipulation will be found in professional Note No. 34, published by the Meterological Office; while the theory, whereby the observations of the tilt of the gun-barrel, as a function of the time of absence of the ball, are made to yield the wind-speeds at successive heights, is set out in Phil. Trans., A, Vol. 223 (1923). The computation of the wind-speed requires a knowledge of the resistance of the air to the motion of the sphere. It appeared desirable to make new experimental determinations of the resistance under conditions similar to those used in measuring the wind, in spite of the fact that by working in the open air one encounters the characteristic difficulty which distinguishes wind measurements from almost all the measurements made in laboratories—namely, the difficulty produced by the gusts and squalls.

Two methods have been employed. The first is a comparison of the projectile with the instrument at present regarded as the standard wind-measurer for the upper air, that is to say, with the pilot-balloon. The comparison is effected by observing the time of absence of the projectile, and the tilt of the barrel from the vertical which is necessary to bring the falling sphere back to the gun ; by observing the wind with a pilot-balloon ; and then comparing the two by the aid of the formulæ developed in the account of the theory.(1)

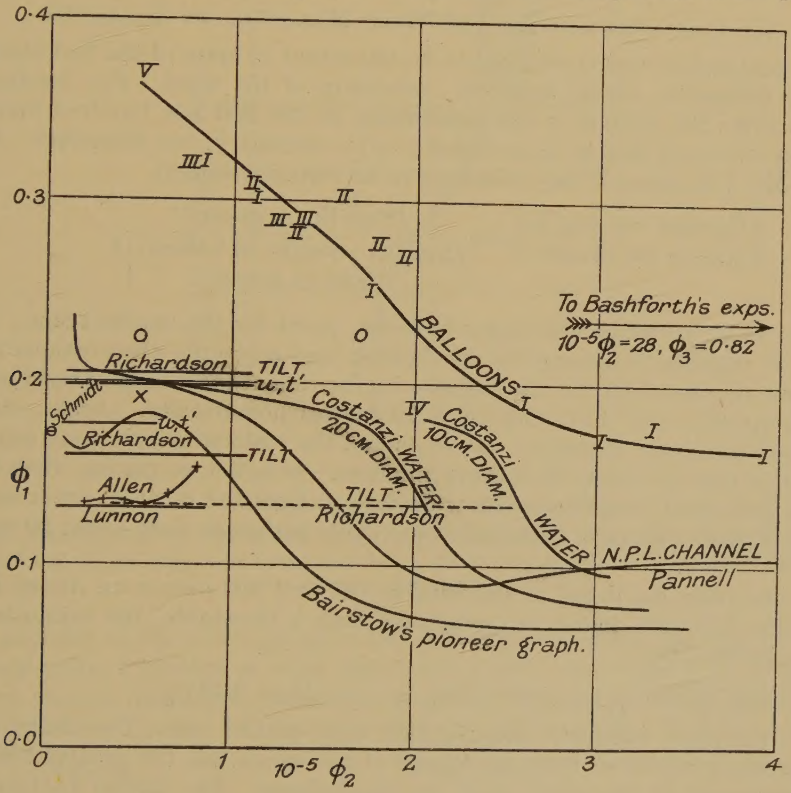

FIG. 1.

Roman numbers represent balloon observations, thus :—
I. J. Rouch. II. F. la Porte. III. Cave and J. S. Dines ; these three reduced by Brazier.
IV. W. H. Dines.(6) V. Horiguti. (17)
But the curve may differ from Brazier's averages. Circles represent W. H. Dines'(6) observations by whirling ; × represents his observation on a ball falling from a kite. One horizontal line is broken to indicate uncertainty.

The second method employed has been to observe the speed with which the sphere leaves the muzzle and the time during which it remains absent.

Throughout most of the theory it has been assumed that the resistance is proportional to the square of the speed-relative-to-air. This is, of course, an approximation, and an eye has been kept open for workable improvements, as will be seen in §3, §4 below.

The results of observation are expressed as numerical values of the terminal speeds which the balls would attain if dropped from sufficiently large heights.

§ 2. COMPARISONS WITH THE WIND MEASURED BY BALLOONS.

This comparison was considered to be important in spite of the fact that there are many difficulties arising from the variability of the wind. For instance, the tail-method for the balloon is not satisfactory in the first few hundred metres, so that a rate of ascent had to be assumed ; as is customary, but imperfect. On the advice of Mr. J. S. Dines,(8) the constant q in his formula, namely

$$\left\{\begin{matrix}\text{Upward velocity in}\\ \text{metres per minute}\end{matrix}\right\}=q\frac{\{\text{Free lift in grams}\}^{\frac{1}{2}}}{\left\{\begin{matrix}\text{free lift}+\text{weight of balloon}\\ \text{(both in grams)}\end{matrix}\right\}^{\frac{1}{3}}}$$

was taken to be $q=84$ for the larger balloons, $q=81$ for the smallest size. Again, it is hardly possible to arrange for the balloon and projectile observations to refer to the same portion of air, for the balloon, by the time it reaches the height attained by the projectile, may have drifted 5 kilometres horizontally. Again, owing to the occupations of the observers, the shots and the balloon ascents were sometimes separated in time by as much as forty minutes. It is not surprising, therefore, to find that individual comparisons show deviations from the mean as great as would be found between two pilot balloon ascents made within an hour—say, 20 per cent. from the mean.

For observing the detail of the wind in the first 200 metres an Abney level or a clinometer has been found to be handier than a theodolite, the azimuths being taken by landmarks.

Polished Steel Sphere of Diameter 0·833 *cm. and Mass* 2·347 *g.*

The five most complete observations were picked out. For each occasion attention was confined to that component of tilt which was the greater and to the same component of the wind given by the balloon. The means for the group run thus :—

Projectiles.

Time of absence, seconds ...	6	7	8	9	10	11	12	13	14	15	16
Cosine of Tilt to horizontal ...	(0·030)	(0·035)	0·041	0·048	0·054	0·062	0·071	0·086	0·104	0·115	(0·127)

Pilot Balloons.

Height above gun, metres ...	50	100	150	200	250	300
Component wind, metres per second	2·8	3·7	5·4	6·7	7·3	(8·3)
ζ metres^3 sec.$^{-3}$	60	130	340	640	1,000	1,500

The values in brackets involve some slight extrapolation.

Here ζ is the same component of the cube of the resultant wind speed. The problem next was to find the terminal velocity which harmonised these two sets of data with the equations (6) given in "Theory," p. 362, due regard being paid to the correction at the vertex ("Theory," § 5·4). It was found by trial that 24 metres per second was fairly satisfactory, as Fig. 2 shows.

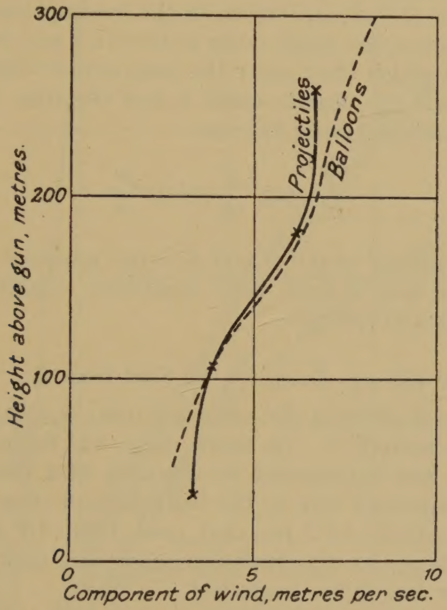

FIG. 2.—COMPARISON OF THE WIND OBSERVED BY BALLOONS WITH THAT BY STEEL BALLS OF 0·833 CM. DIAMETER, WHEN THE TERMINAL SPEED OF THE LATTER IS ASSUMED TO BE 42 METRES PER SECOND.

In addition to the above group of five observations, which have been combined in a group-mean, a number of other observations have been worked out individually and have yielded the results shown in the table below. The individual differences are attributable to gusts. The mean values are collected later on in § 4.

Projectile	Material	Polished steel.						Lead "lethal."	
	Diameter, cm. ...	0·794		0·833		1·826		1·85 over ridges,	
	Mass, grms. ...	2·04		2·347		24·71		1·73 on core, 31·6	
Value of terminal speed which makes projectile agree with balloon, metres per second		39·3	31·4	36	37	81	53	69	56
Subsidiary data	Mean speed of wind component used, metres per second	3	changed sign	2	increasing rapidly aloft	up to 10 at vertex	3	3 to 7	—
	"Naturalised" height attained, $L=$ ...	0·8	2·2	1·6	0·4	0·6	1·2	1·4	0·8
	Correction at vertex as per cent. of quantity corrected	2	4	1	11		1	2	8

§ 3. CONSTANTS DETERMINED FROM TIME OF ABSENCE AND MUZZLE SPEED.

An advantage of this method is that the effect to be measured is a large one, for the observed time of absence is very different from what it would be in vacuo, as will be seen from Fig. 3. Another advantage is that any scatter produced by gusts is quite small. From the point of view of standardising a wind-measurer, however, a disadvantage is that any errors, in the assumption that the resistance is proportional to the square of the speed, come in here in a way which is not necessarily the same as the way in which they enter the comparison with balloons.

The relation between the muzzle-speed u and the time of absence t' is shown in " Theory " (§4·2, equations 14 and 1) to be

$$\frac{t'g}{k}=\tan^{-1}\frac{u}{k}+\sinh^{-1}\frac{u}{k}. \qquad (1)$$

where g is the acceleration of gravity, and k is the terminal speed which we seek to determine. When u and t' have been measured, equation (1) fixes k. It is applied below to two sizes of sphere.

Polished Steel Sphere, 0·437 *cm. Diameter, as Shot from the "Daisy" Air Gun.*

The observed time of absence did not vary from one shot to another by more than a few tenths of a second(15). Its mean value was found to be 10·1 seconds.

The muzzle speed was determined by shooting at a ballistic pendulum. The latter was made of compressed cork as the balls did not stick in wood. The result, which appeared to be reliable to 2 per cent., was $1{\cdot}23\times10^4$ cm. sec.$^{-1}$. This value may be a trifle too large, as the ballistic pendulum was used with the barrel horizontal. This error has been neglected.

On inserting these values in equation (1) it is found that $k=28{\cdot}0$ metres per second.

Polished Steel Sphere of Diameter 0·833 *cm., as Shot from a No.* 3 *Bore Gun.*

Here explosives were used as propellants, and it was found that the relation between the charge of powder and the muzzle speed was not at all definite, so that it was necessary to determine the muzzle speed for the individual shot whose time of absence was measured. The muzzle speed was found from the kick of the gun after due calibration. Calm days being scarce, it was necessary to vary the tilt to balance the wind and at the same time to measure the kick. For this purpose the table of the Cartesian mount* was made of one of the pans of a Roberval's balance in the form of an Avery weighing machine. The muzzle was constrained to move in a vertical line by a long light stiff lever. The mechanics of this arrangement involved a special investigation. The observation gives the momentum communicated to the gun. A part of this, too large to be ignored, is balanced by the momentum of the products of combustion. To determine the latter the classical procedure of Robins was followed, the gun being made into a ballistic pendulum and the shot being caught in another pendulum.

After thus finding the true muzzle speed, it was plotted against the time of absence. The result is shown in Fig. 3 for 21 shots individually. On drawing a smooth curve through the observations, the speeds in the second column of the table below were read off. The third column of the same contains the terminal speed deduced from the data in the first and second columns if the resistance is proportional to the square of the speed by using equation (1).

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* So called because it enables Cartesian co-ordinates to be read off directly.

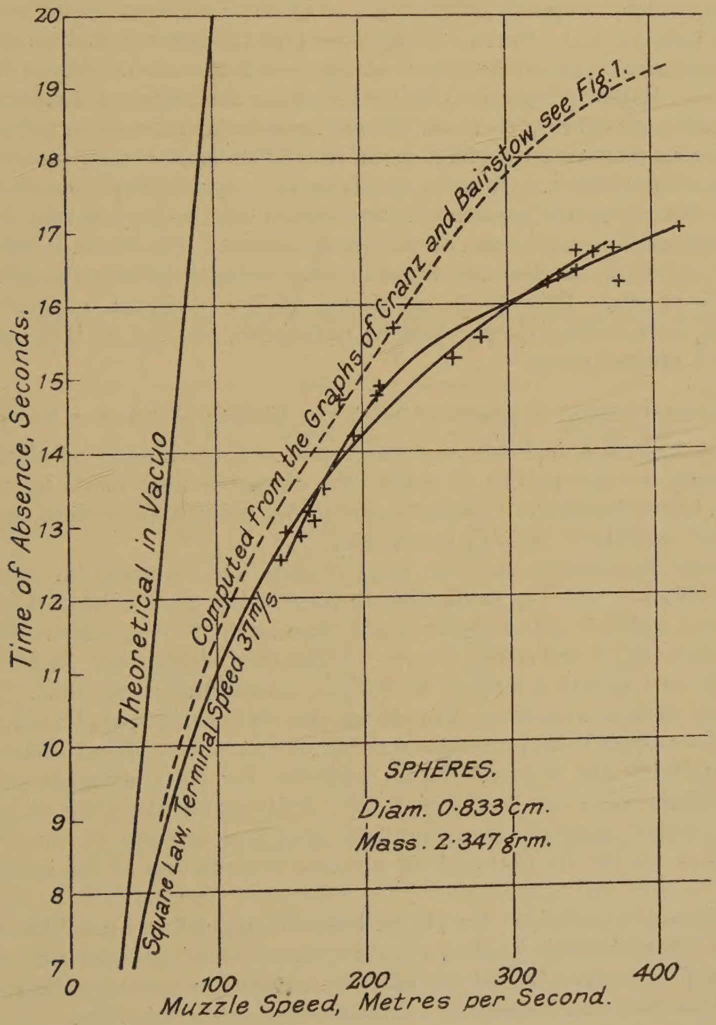

FIG. 3.

Time of absence. Secs.	Observed muzzle speed. m/s.	Deduced terminal speed. m/s.
15	224	37·6
14	186	37·0
13	157	36·0

One of the lines on the diagram shows that the terminal speed of 37m/s. fits passably well with the whole range of these observations, including even those at muzzle speeds equal to that of sound.

In the hope of obtaining a better agreement with observation, certain deviations from the square law were here introduced into the theory. Bairstow (10) gives a firm curve, which is reproduced in Fig. 1. From this curve a table was prepared showing the resistance as a function of the speed for the sphere 0·833 cm. in diameter. It was also necessary to take account of the speed of sound. Now Cranz, in his "Lehrbuch der Ballistik," gives a diagram showing this effect as a multiplier to the resistance, which is unity up to about 160 m/s. and then increases to 2·68 at 400 m/s. Accordingly the resistances derived from Bairstow's curve were increased in the ratio of Cranz's multiplier. Then by an elaborate numerical process—here omitted for brevity—the dynamical equation for vertical motion was integrated so as to give the time of absence as a function of the muzzle speed. The result is shown by the broken line in Fig. 3. It does not fit with observation as well as the simple square law with $k=37$ m/s. This result, as shown on the diagram, suggests that the decrease of φ_1 with increasing φ_2, though appreciable, was not so large as indicated by Bairstow's pioneer curve.

Further particulars relating to spheres of 0·833 *cm. diameter shot from a No. 3 bore gun.*

(1) *Momentum Lost in Products of Combustion.*—Experiments to determine the muzzle velocity of the sphere of 0·833 cm. diameter were made by placing the gun with its barrel horizontal, converting it into a ballistic pendulum, and firing at a target which was also a ballistic pendulum.

The recoil momentum of the gun exceeds the momentum given to the target by an amount which is partly due to the moving gases dispersed in the intervening air, and which it is the object of the experiment to determine. The method is so well known that it will suffice to refer to Routh's Elementary Rigid Dynamics, §§ 121 to 125, and to add a note as to various details and corrections. The target was provided with a cork face, into which the shot easily penetrated. The gun was hung in two equal V-shaped loops of string, so that it translated without rotation thereby simplifying the theory. As an indicator for the maximum displacement of each pendulum there was used a steel wire (knitting needle) lying on wood. The needle was pushed along by the pendulum, and was brought to rest by friction. The corrections for the friction and for possible overrunning of the indicators were determined.

The trigger was pulled, at Mr. H. W. Baker's suggestion, by a "Bowden" wire consisting of a flexible cable inside a flexible tube of coiled wire, as used on bicycles. The strain in this system affected the pendulum slightly ; but by pulling the trigger very slowly the effect was reduced to a small alteration of static zero.

The geometry of the indicators led to some little corrections. An allowance was made for the loss in velocity of the ball between the muzzle and the target.

When all these corrections had been attended to, an accuracy of 1 per cent. appeared to be attained. The results of the last made and most accurate observations are set out in a table and diagram. They are supported by a number of preliminary observations. It is evident that—

(I.) The muzzle velocity for a given charge of fulminate and powder may vary by 20 per cent. on either side of its mean.

(II.) With a perfectly clean barrel the muzzle velocity is low. As the barrel becomes gradually fouler the muzzle velocity at first increases. This is in agreement with the experience of the makers of sporting cartridges, for they turn over the ends of the cartridges in order to introduce resistance so as to make the powder explode forcibly. However, if the charge of powder was only 0·014 gram the ball has been known to stick in a foul barrel and remain there, whereas a very clean barrel has given a long time of vertical flight.

(III.) Excluding the case of a very clean barrel, the momentum of the products of combustion increases with the velocity of the ball, as the diagram shows.

The above refer to a Hoffmann steel ball 21/64″=0·833 cm. diameter, fired by rim fire cartridges made by S.F.M., of Paris. They contain 0·16 gram of a silvery looking fulminate, which left a yellow solid after firing. The black powder was weighed in. No wad was used. The clearance between the ball and a clean barrel was such that the ball just slid easily but did not rattle from side to side.

The pendulum experiments were carried out with the help of Mr. J. A. Gaunt.

Record of Shots in Order of Time to Show Progressive Effect of Fouling.

Barrel.	Mass of black powder.	Mass of fulminate.	Momentum of ball at muzzle.	Momentum of gun exceeds momentum of ball by	Speed of ball m/s.
	grm.	grm. cm.	grm.cm.sec^{-1}	grm.cm.sec^{-1}	
Clean and dry ...	0·31	approx 0·16	4·65	1·62	198
Less so	0·115	0·16	5·37	0·81	229
		Barrel cleaned here.			
Clean & slightly oily	0·31	0·162	2·87	1·94	122
Less clean	0·115	0·157	3·42	0·48	146
Less clean	0·31	0·160	5·26	2·08	224
Rather foul	0·115	0·161	3·89	0·43	166
Rather foul	0·31	0·158	7·02	2·38	299
Foul	0·115	0·158	4·83	0·75	206
Foul	0·31	0·165	8·87	2·70	378
Foul	0·115	0·170	3·47	0·35	148

These measurements are set out on the attached diagram.

(2) *Time of Absence and Momentum.*—For measuring the kick of the gun we arranged a weighing machine of Roberval's pattern. The butt rested on one pan, and on the other pan the deadweight of the gun was counterpoised.

The chief advantage of the Roberval balance was that its large flat pan allowed room for horizontal manœuvre of the butt, the muzzle being constrained by a lever to move vertically. Such manœuvre is necessary in order to balance the wind.

Ideally the equilibrium should have been neutral and the sensitivity infinite. Then an additional mass was put on to hold the pan, which was opposite to the gun, hard down on its stop. When the gun was fired this extra mass rose, and the whole weighing machine moved under the constant acceleration due to this weight.

The maximum excursion was recorded by a short piece of steel wire sliding in a brass groove. With a sliding piece having such a very small mass it was possible to apply to it a friction which was sufficient to prevent any over-running, and yet had a negligible effect on the balance as a whole. When the balance had fallen back, the wire was securely clamped, and its excursion was then measured by a micrometer screw. This recording mechanism, which was suggested by Mr. W. H. Dines, and accurately made by Mr. H. W. Baker, worked very nicely.

The hard stop on which the loaded pan rested, gave a definite zero from which the excursion was measured. But the hard stop carried with it a serious disadvantage. It will be remembered that when one is using ballistic pendulums or ballistic galvanometers, the whole momentum should ideally be communicated to the moving system while it is under the action of no restoring force. In the present instrument a large restoring force is always present, and there is a corresponding correction which we shall analyse below.

A photograph of the balance is shown in Fig. 5. Although composed of many members its position is completely specified by the height of the pan opposite to that on which the gun rests. Let z be this height. Then we may define an "equivalent mass" m_1 to be such that $\frac{1}{2}m_1(dz/dt)^2$ is the kinetic energy of the system. The gun translated almost without rotation. The pans also moved parallel to themselves and very nearly vertically.

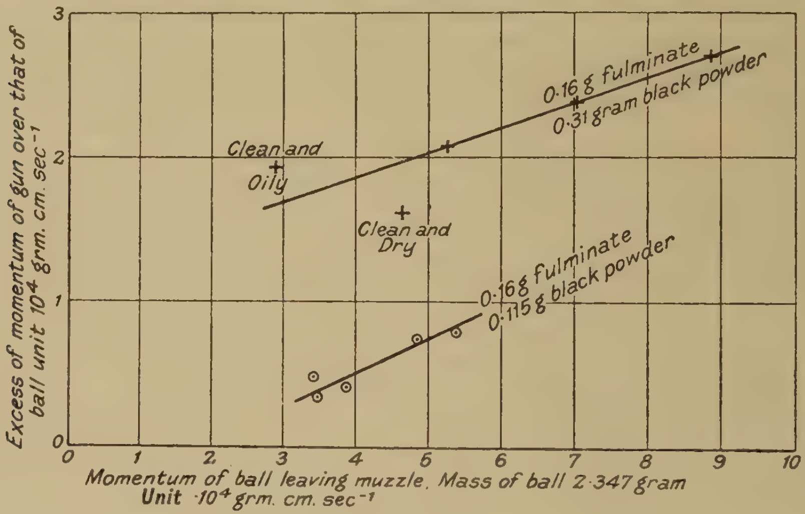

FIG. 4.

Therefore

$$m_1 = (\text{mass of pans}) + (\text{mass of gun}) + (\text{masses of all counterpoises})$$
$$+\left(\frac{\text{distance between adjacent knife edges}}{\text{radius of gyration of beam}}\right)^2 \times (\text{mass of beam}).$$
$$+(\text{similar terms for other rotating parts})$$

These various parts were separately measured and so m_1 was found to be 32·1 kilograms.

Now an extra mass m_2 of 1 kilogram was placed on the pan opposite to the gun so as to hold that side hard down on its stop.

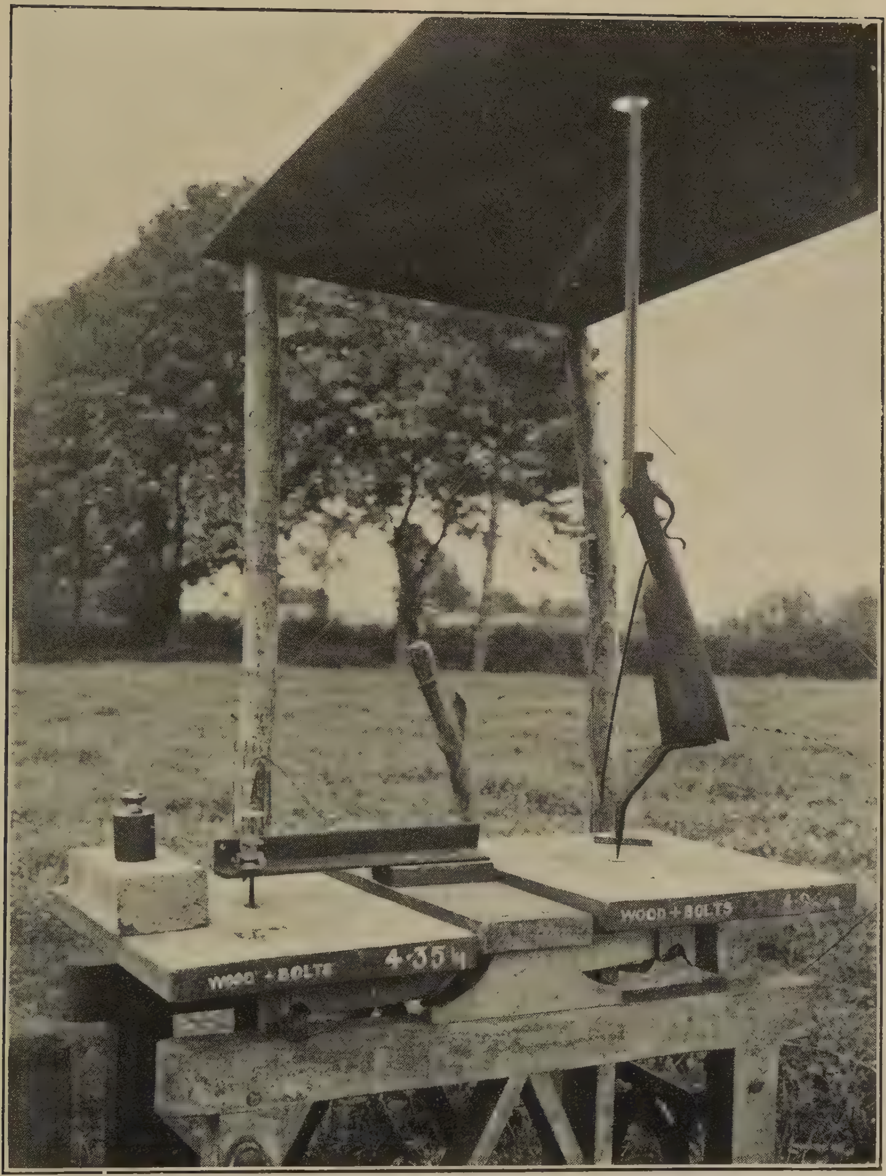

FIG. 5.—ARRANGEMENT FOR MEASURING KICK OF NO. 3 BORE GUN.

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The equation of motion of the system, when not on its stop, is then obtained by equating changes of kinetic and potential energy, thus

$$\frac{d}{dz}\left\{\tfrac{1}{2}(m_1+m_2)\left(\frac{dz}{dt}\right)^2\right\}=-m_2g. \quad \ldots \quad (1)$$

During the ejection of the ball this equation must be modified by an additional term representing the mass-acceleration of all the ejecta. Let σ be the distance which the ball has moved up the barrel and let $m_3d^2\sigma/dt^2$ be the force acting on the ejecta so that m_3 is a sort of equivalent mass. It is true that m_3 may then be variable, especially just as the ball leaves the muzzle. We shall treat m_3 as constant, for the error thereby introduced effects only a correction, not the main term. Thus during ejection through a barrel pointing at a zenith-distance ζ

$$(m_1+m_2)\frac{d^2z}{dt^2}+m_2g=m_3\frac{d^2\sigma}{dt^2}\,.\cos\zeta. \quad \ldots \quad (2)$$

Let λ be the length of the barrel, τ the time which the ball spends in traversing it, and u the speed with which it emerges from the muzzle, and let z' be the corresponding part of the motion of the balance. Then by integrating (2) it is found that

$$z'=\frac{\cos\zeta\,.\,m_3\,.\,\lambda-\tfrac{1}{2}m_2g\tau^2}{m_1+m_2} \quad \ldots \quad (3)$$

At this point the velocity of the balance is

$$\left(\frac{dz}{dt}\right)_{t=\tau}=\frac{\eta\,.\cos\zeta-m_2g\tau}{m_1+m_2}. \quad \ldots \quad (4)$$

where η is the total momentum given to the ejecta. This statement is not affected by the variability of m_3.

Afterwards the balance makes a further free excursion from z' to z'' at which latter point its velocity vanishes, so that z'' is what we measure. By integrating (1) and inserting (4) it is found that

$$z''-z'=\frac{(\eta\cos\zeta-m_2g\tau)^2}{2m_2g(m_1+m_2)}. \quad \ldots \quad (5)$$

Eliminating z' from (3) and (5), and rearranging, it is found that

$$\eta\cos\zeta=\sqrt{2m_2g\,\{(m_1+m_2)z''+m_3\cos\zeta\,.\,(u\tau-\lambda)\}} \quad \ldots \quad (6)$$

The term in $(u\tau-\lambda)$ is a correction which depends on the character of the motion in the barrel. If the speed of the ball in the barrel increases without ever decreasing, then $u\tau>\lambda$. If also the acceleration of the ball is greatest at the beginning of its motion and continually decreases $u\tau<2\lambda$. Taking the mean of these two limits we may put $u\tau=1{\cdot}5\lambda\pm0{\cdot}5\lambda$. Inserting this in (6) above we obtain

$$\eta\cos\zeta=\sqrt{2m_2g\,\{z''(m_1+m_2)+\cos\zeta\,.\,m_3(\tfrac{1}{2}\lambda\pm\tfrac{1}{2}\lambda)\}} \quad \ldots \quad (7)$$

Formula (7) has been used to find η and then from η the muzzle-velocity has been deduced by reference to the experiments on the momentum of the products of combustion. The smoothed results of 22 nearly vertical shots are as follows :—

Time of absence, seconds.	Muzzle velocity.	Uncertainty arising from $\pm\frac{1}{2}\lambda$ in (7).
	Metres per second.	
17	410	±4
16	300	±5
15	223	±7
14	185	±9
13	158	±12

The time of absence is reckoned from the muzzle to the field at a level 2 metres lower.

Mass of sphere 2·347 gram ; diameter 0·833 ; surface polished steel.

Mean air density ... ... $1{\cdot}24 \times 10^{-3}$grm. cm.$^{-3}$ at gun.
Mean air temperature ... 285° A at gun.
Mean tilt of gun ... ... $=0{\cdot}05$ radians to vertical.

As a check on the standardisation of the balance, 16 experiments were made by dropping on to one pan definite masses of putty from heights not exceeding a metre. The observed values of momentum differed from those derived from an appropriate theory by *at most* −9 per cent. and +10 per cent., and on the average by 1 or 2 per cent. There was again some correction, arising from the slowness of the shock, but in this case it could not be estimated and was omitted.

§ 4. SUMMARY AND CONCLUSION.

The new observations described in the present Paper, when expressed in terms of φ_1 and φ_2, which have been defined in §1, run as follows :—

Polished Steel Spheres.

Diam.	Mass. Gram.	Observed terminal speed. Metre sec.$^{-1}$	Method.	Air Density. Grm./litre.	φ_1.	Range* of $10^5\varphi_2$.
11/64″ =0·437 cm.	0·336	28·0	*u* and *t′*	1·25	0·176	0·04 to 0·6
5/16″ =0·794 cm.	2·04	35·3	tilt	1·24	0·205	0·1 to 1·1
21/64″ =0·833 cm.	2·347	37 41	*u* and *t′* tilt	1·22 1·24	0·199 0·160	0·1 to 1·1
23/32″ =1·826 cm.	24·71	67	tilt	1·20	0·134	0·35 to 2·5

Mean of φ_1 for steel balls, giving treble weight to the reliable set 0·160, is 0·171.

Lead "Lethal" Sphere with Projecting Ridges.

1·85 cm. over ridges, 1·72 on core	31·6	62·5	tilt	1·22	0·190	0·4 to 2·5

* Values of φ_2 corresponding respectively to half the terminal speed (below which gravity rather than resistance is the control) and a mean muzzle-speed of 200 m/s.

These are represented on Fig. 1 by lines parallel to the φ_2 axis. The parallelism of these lines is partly a confession of inability to distinguish one part of the line from another, but not entirely so, for Fig. 3 shows that the square law of resistance gives a tolerable fit throughout the working range.

Looking now at Fig. 1 in general, it appears, at first sight, to be a bewildering confusion. On consideration, however, certain regularities emerge :—

For pilot balloons the values of φ_1 are markedly higher than all the rest in the range $1 < 10^{-5}\varphi_2 < 2$. Now pilot balloons are not true spheres ; their defects, being visible to the naked eye, probably amount to 2 to 5 per cent. on the diameter. Furthermore, on being observed through a telescope when rising, they are seen to roll to and fro, and usually to rotate about a vertical axis. That the rolling increases the resistance, and so φ_1, is shown by an experiment of Cave and J. S. Dines (8) in which adding weight to a pilot balloon, so as to make it roll less, caused it to rise faster. Horiguti (17) observed the same phenomenon. Thus pilot balloons are not comparable with wind-channel spheres which are not allowed to roll. Yet for both φ_1 decreases as $10^{-5}\varphi_2$ increases from 1 to 3.

On the contrary, the wind-channel experiments, which give the lowest values of φ_1, are from balls completely prevented from rotation. The phenomena in wind-channels, however, are suspected (cp. Pannell, Brazier) of being complicated by a turbulence differing from that of the open air, and may further be affected by the necessity for some solid support for the ball.

The ball-bearing spheres are probably more perfect geometrically than any of the others, and have been used without any supporting brackets. They have been free to roll. The media have been free from *artificial* turbulence, except perhaps the air in some of the pit-shafts used by Lunnon. The observations on these spheres by Lunnon in air agree admirably with those of Allen in water. The observations recorded in the present Paper lie well in the middle of the general scatter, making it plain that φ_1 is much less than for balloons and yet higher in the range $1{\cdot}2 < 10^{-5}\varphi_2 < 2{\cdot}5$ than for the wind-channel experiments. The present observations on spheres shot up to heights of several hundred metres tend rather to agree with Costanzi's observations on spheres towed in non-turbulent water. This suggests that the turbulence of the upper air has decidedly less effect than the turbulence of experimental channels. The author (19) has published a collection of measurements of the turbulence of the upper air, and it would be interesting to make the comparison.

For the restricted purpose of measuring the wind by shooting spheres upward, the fixed value $\varphi_1 = 0{\cdot}17$ has been used provisionally. The deduced wind-speeds, being proportional to $\sqrt{\varphi_1}$, have a fractional uncertainty roughly half that of φ_1.

§ 5. PERSONAL.

These experiments were made as an official research at Benson Observatory in 1919-1920, and are now communicated by permission of the Director of the Meterological Office. Mr. H. W. Baker, who was assisted by Mr. B. C. Lewis, took many of the balloon-observations, and carried out all the fine mechanical work. Mr. J. A. Gaunt, a visitor, gave much help with the pendulum observations. I should like also to express my great indebtedness to Mr. W. H. Dines, F.R.S., who was in charge of the Observatory, and to Sir Napier Shaw, F.R.S., who was then Director.

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DISCUSSION.

Mr. F. J. W. WHIPPLE congratulated the author on having carried out a very useful piece of work, which he hoped would lead to the permanent adoption amongst meteorologists of methods such as that described. At present no use seemed to be made of the latter for practical purposes, although there is a crying need for more information relating to the atmosphere at moderate heights, particularly during cloudy or foggy weather. Not only the strength of the wind, but the temperature could probably be ascertained by similar methods, since, as the author has recently mentioned to the Meteorological Society, the functions φ_1 and φ_2 depend on temperature through the viscosity and density terms which they contain. The speaker suggested that better conditions for the experiments would exist in the middle of a lake, where an observer on an island could see the splashes instead of listening for the fall of the projectiles. He also suggested that if the projectiles were explosive, with a time fuse, sound-ranging methods could be employed.

Mr. J. E. CALTHROP inquired whether the discrepancies between the results obtained by the author's method and those obtained from balloons might not be due to the spin of the projectiles.

The AUTHOR, in reply to the discussion, said that it was cheering to find that a meteorological authority like Mr. Whipple thought that the method of measuring wind by shooting spheres upward would be of use. He agreed that water is the best target. In reply to Mr. Calthrop's question, he explained that the barrels were not rifled.

VI.—INVESTIGATIONS OF PIEZO-ELECTRIC EFFECTS WITH DIELECTRICS.

By K. R. Brain, *B.Sc., Lecturer in Physics, St. John's College, Battersea.*

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(*Communicated by S. Marsh.*)

Abstract.

The Paper describes experiments on the piezo-electrification of dielectrics. The dielectrics examined were ebonite, glass, hornoid, sealing wax, rubber, celluloid, and hard paraffin. The relation of quantity to load was determined ; with sensitive specimens the magnitude of the charge is of the same order as that found by Curie with crystals.

Fatigue and hysteresis effects in these dielectrics were established. Experiments on cubes showed a dissymmetry in results, suggesting an irregularity of structure.

The general similarity of the behaviour of these dielectrics to that of crystals leads to the assumption that they possess a quasi-crystalline character, and the effects are explained on this assumption, which has been verified by X-ray photographs.

I. Introduction.

THE electric excitation of crystals by pressure has been known for years, due to the work of Curie and followed by Voigt, Röntgen, and Kelvin.

A. W. Ashton* found that if a sheet of rubber was laid between two brass plates which were connected to the quadrants of an electrometer, and a 2 lb. weight allowed to fall from a height of a few inches on the upper brass plate, the needle moved quickly to the right, but immediately returned and showed a steady deflection to the left.

If the sheet was stretched there was again a deflection to the right, and when the electrometer was discharged and the sheet allowed to collapse, there was a deflection in the opposite direction showing a potential difference between the plates of more than 10 volts.

This suggested that piezo-electric effects might be shown by dielectrics which were not, at least obviously, of crystalline character. With a view of testing this, experiments were made by Marsh in 1913-1914 on rubber, ebonite, celluloid, and hard paraffin, and these showed conclusively that it was possible to electrify these dielectrics by pressure, and the results indicated that fatigue and hysteresis effects were present. Owing to insulation difficulties the reduction of the observations was exceedingly tedious and the work was discontinued.

In the following Paper the subject has been resumed, particular attention being paid to the improvement of the insulation and other adjustments of the apparatus (with such success that " creeping " and " leakage " were got rid of), and each substance has been examined in a systematic manner.

II. Apparatus.

The apparatus used is simple in design, and consists of three separate pieces, each being mounted upon perfectly rigid supports—(Fig. 1).

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* Phil. Mag., Aug. (1901).

No. 1.—The electric lamp and scale, fixed to the table to render calibration constant.

No. 2.—The device for applying pressure to the specimens under observation. This consists of a stout copper plate P, which supports the specimens S 1 and 2. A metal cone M ensures a normal and even distribution of the pressure. The stirrup of a scale pan balances on the apex of the cone, the pan hanging about 18 in. below the table. A pulley enables the operator to raise or lower a weight upon the scale pan at will. The whole of the apparatus is surrounded by an earthed sheet tin casing. The upper plate, the metal cone and the scale pan are all earthed by connections with the metal casing. Between the specimens is a thin copper sheet,

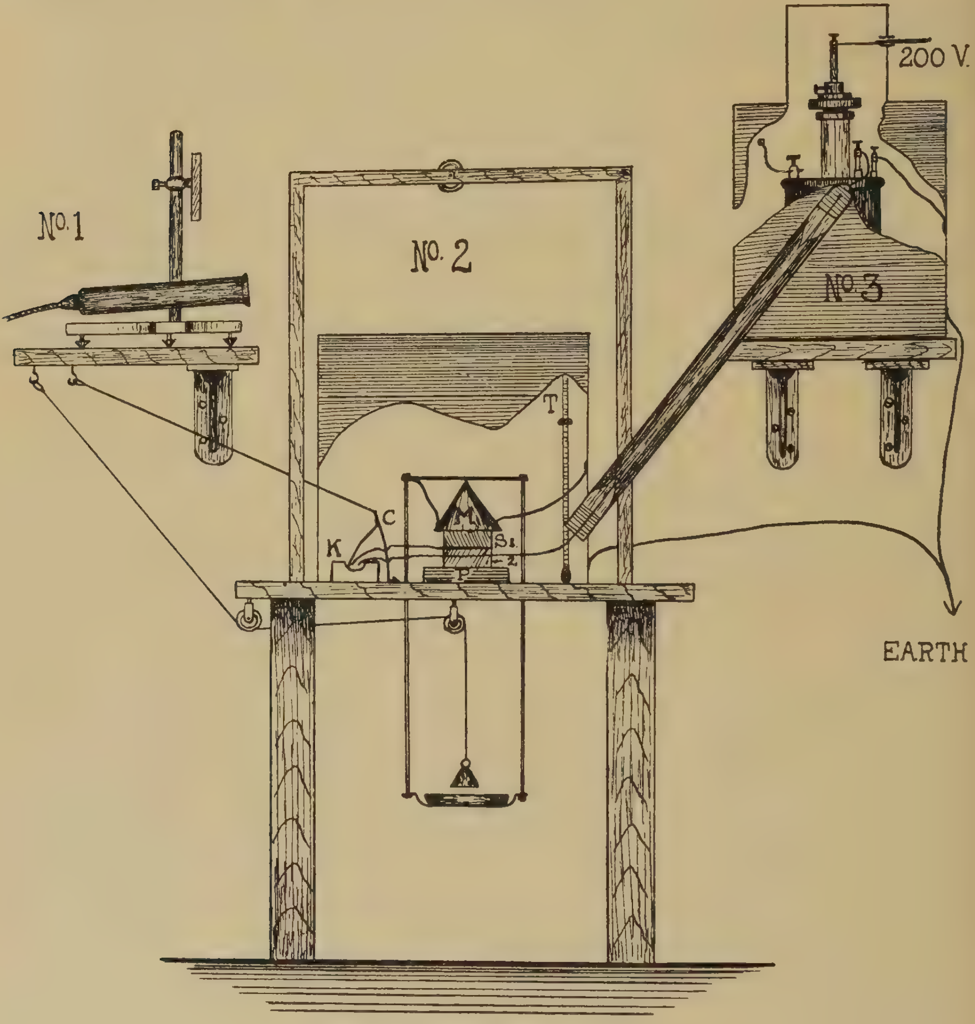

FIG. 1.

which carries a long arm dipping into the mercury cup *K*. A spring *C* earths this cup when desired. The mercury cup is connected with the electrometer by a copper wire passing through an earthed tube.

No. 3.—A very sensitive Dolezalek electrometer, of modern type, is enclosed in a metal casing, which, together with one pair of quadrants and the case of the electrometer, is earthed.

The sandwich form involved the use of two similar specimens, and in order to deal with a single specimen the following modification is employed. The dielectric under observation is placed upon the copper plate—(Fig. 2). On this is placed the copper sheet, connected with the mercury cup, being kept in position by a stout plate of iron (466 grm. wt.). A wooden gallows supports a pulley above the iron, and this permits a weight to be lowered.

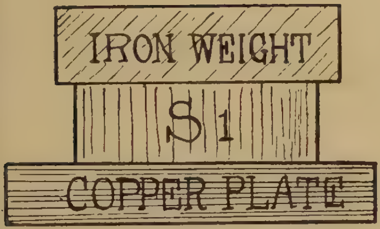

FIG. 2.

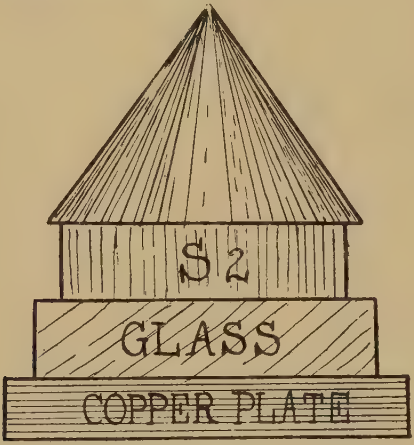

FIG. 3.

III. METHOD OF OBSERVATION.

The electrometer was adjusted until the mechanical and the electrical zeros were coincident. The voltage of the needle and the reading of the thermometer were obtained. After noting the stationary reading when *C* was released, the load was put on and the final reading taken. The difference gave the deflection due to the loading of the specimens. The specimens were earthed and the stationary reading when *C* was released was again taken. The load was removed and by noting the final position the deflection due to the unloading of the specimens was obtained.

IV. CONSTANTS OF THE APPARATUS.

The electrometer was calibrated in the usual manner.

The capacity was measured by making it share a charge with a standard condenser. The mean of the readings gave the capacity as 237·81 E.S.U.

The specimens and the copper sheet between them were found to have a negligible effect on the capacity of the system. The insulation was very good, and steady deflections made a correction unnecessary.

The instrument took about a minute to register a steady reading after receiving a large charge.

EBONITE.

V. Sandwich Form.

In the preliminary experiments (after establishing that the effect was present) the object was to find how the surfaces charged under pressure. Each specimen (1·25 × 1·25 × 0·5 in.) was marked to show corresponding faces as directive properties were expected. To deal with a single specimen the form of Fig. 3 was adopted, the effect of the glass being considered negligible. This assumption was found to be unjustified, and to avoid the introduction of another insulator the sandwich was formed of two specimens of ebonite. During the first readings a marked drift was observed on breaking earth contact. This was eliminated by soldering all earth connections and cleansing the specimens with ether before assembling the apparatus, care being taken not to handle the specimens by faces to be examined.

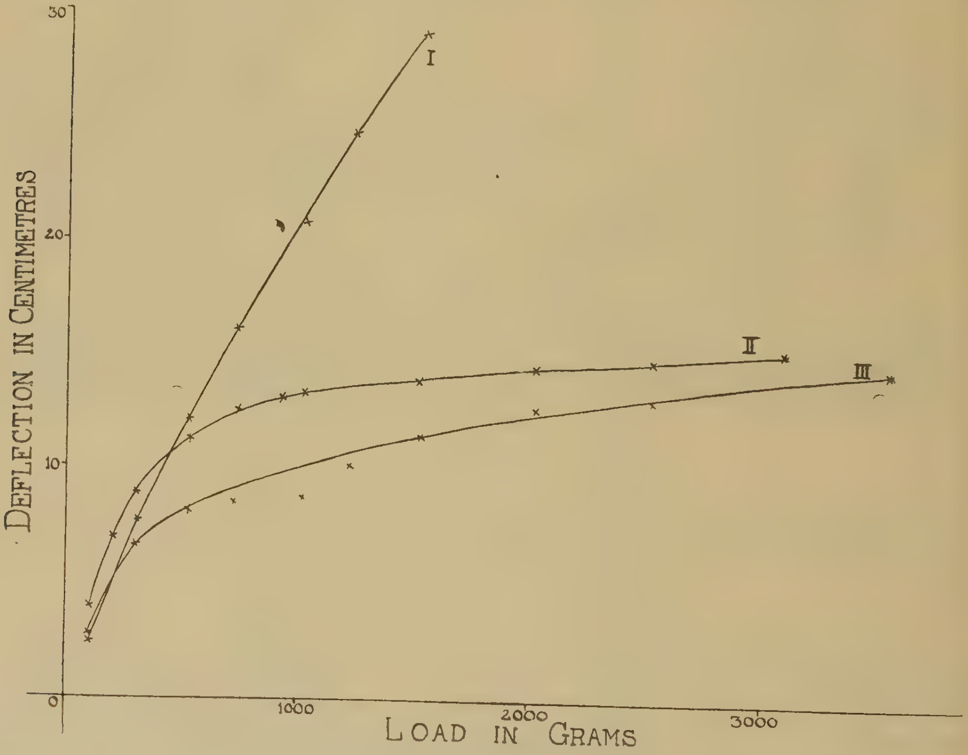

Fig. 4.

There is little difference between loading and unloading deflections, except for a reversal of charge, so a mean of the readings is taken for graphical representation.

Fig. 4 shows three sets of readings on the same specimens of ebonite with an interval of two days between each. The specimens were not under stress in the interval. They suggest a fatigue effect which will be discussed later.

The large deflections are characteristic of certain specimens; others do not show the effect to nearly so marked a degree, their behaviour apparently depending largely on their previous history with regard to pressure or stresses of any nature. When each of the specimens of the above form were reversed to bring opposite faces together the deflections were very much smaller and of opposite sign on loading and unloading to what they were in the previous case.

A summary of these results is shown in the table below.

	↓ 4. ↑ 3.	↑ 4. ↓ 3.	↓ 4. ↓ 3.	↑ 4. ↑ 3.
LOADING... ...	Left	Right	Left	Right
	11·5	5·5	1·3	2·5 cm.
UNLOADING ...	Right	Left	Right	Left

These results indicate that while the faces carry opposite charges they are not equal in magnitude. This led to the advisability of using one specimen and loading from above.

VI. SINGLE SPECIMEN. OVERHEAD LOADING.

For this purpose the framework was fixed over the casing containing the platform. As the weight, when lowered, had to form part of the insulated system a means was sought to insulate this. A short piece of unspun silk joined the weight to the extremity of a pencil of sealing wax while to the other end was attached the operating cord.

The silk and the sealing wax were both found to charge owing to the tension, the latter to such a degree that it had to be removed and a longer length of silk employed. The silk was always kept thoroughly dry or erratic readings were obtained.

Sets of consistent readings were obtained and appear quite satisfactory as regards the behaviour of the deflections. The charging of the silk was verified by taking a bare copper wire and winding it round the fibres, and then allowing it to dip into the mercury cup. On subjecting the fibres to a tension a deflection of the needle was obtained.

The actual effect of the silk at the point of suspension of the weight cannot be obtained as a dielectric cannot be employed owing to its piezo-electric properties.

Thus the results obtained include the effect of the silk. The curves of Fig. 5. showing the relation of charge to load, are typical and indicate that opposite faces (under pressure) charge similarly. These curves necessarily include the effect of the silk, so little importance is attached to the readings. When specimens examined as above were put into sandwich form the sum of the charges was not obtained. Hence an attempt was made to obtain readings for a single specimen without introducing another dielectric.

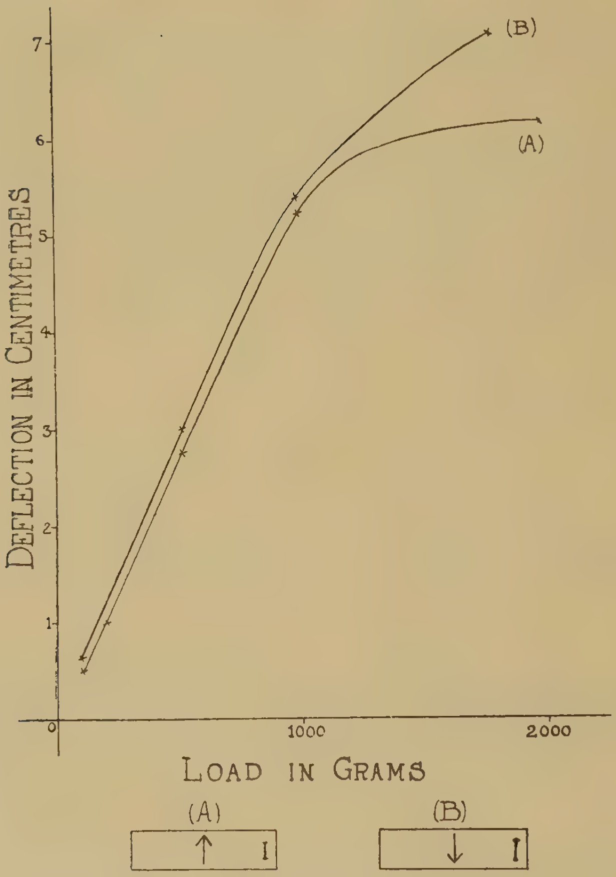
DEFLECTION IN CENTIMETRES
7
6
5
4
3
2
1
0
1000
2000
LOAD IN GRAMS
(B)
(A)
(A)
(B)
I
I

FIG. 5.

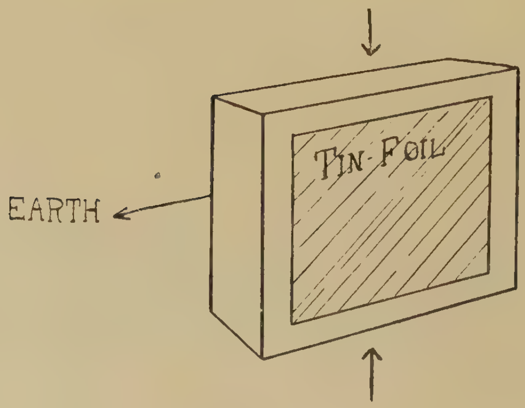
EARTH
TIN-FOIL

FIG. 6.

The usual method of applying pressure as to the sandwich form was adopted, but a single specimen was used, and one examined the charges which appeared on the vertical faces (Fig. 6). The deflection was of opposite sign on loading, as would be expected, since contraction on the direction of pressure would produce an elongation in the direction at right angles. The results of these experiments indicate that charges are of different magnitude, on opposite faces, but of the same kind.

VII. CUBE OF EBONITE.

If the substance under consideration possesses a perfect uniformity in its physical structure one would expect that the charges on the various faces would be equal. It seems quite feasible that some directive property might have been given to the specimen in the course of its manufacture. In order to determine whether the piezo-electric phenomena would indicate this hypothetical irregularity in structure, a cube of ebonite was taken and the charges examined upon each face in turn, all the other faces being earthed.

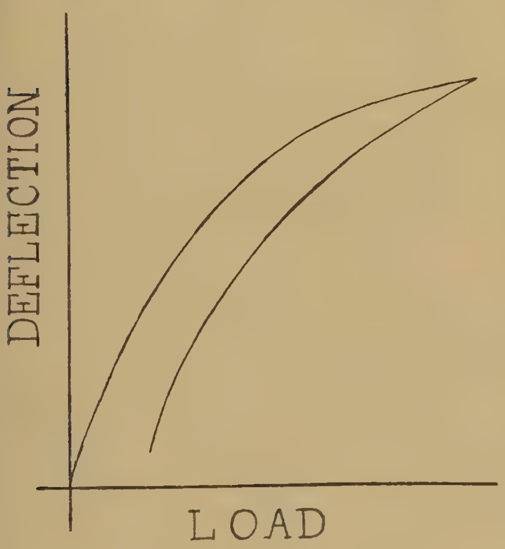

FIG. 7.

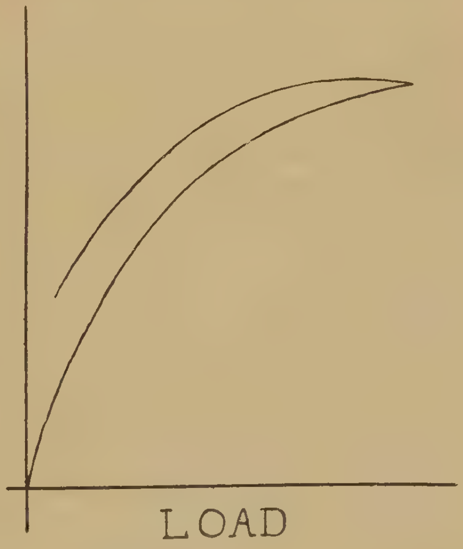

FIG. 8.

In general, the deflections obtained were quite small (0·2 cm.), although they were quite definite and accurate. With each cube a sensitive face was usually found giving 2-3 cm. deflection. The observations recorded support the hypothesis that the dielectric substances examined have an irregular structure.

VIII. FATIGUE AND HYSTERESIS EFFECTS.

Throughout the work on ebonite the fact has been evident that, in general, the deflections decrease as loads are applied continually. The specimen is fatigued and this effect is more evident the larger the load applied.

To test this, specimens were taken in the sandwich form and the same load applied continually. With fresh specimens the deflections generally increased at first and gradually decreased. The specimen does not recover its initial sensitiveness even after a period of six months. Owing to this effect, when loads are applied in increasing order of magnitude and then decreased, the curves obtained are not coincident, but as shown (Fig. 7), the latter being below the former.

The reverse of this was shown with a few specimens examined (hysteresis, Fig. 8), the curves on decreasing the loads being above that obtained on increasing the loads.

When the fatigue effect of these specimens was tried the deflections increase with each application of the load and only after many applications (and weight left on all night in some cases) did they fall off (Fig. 8A).

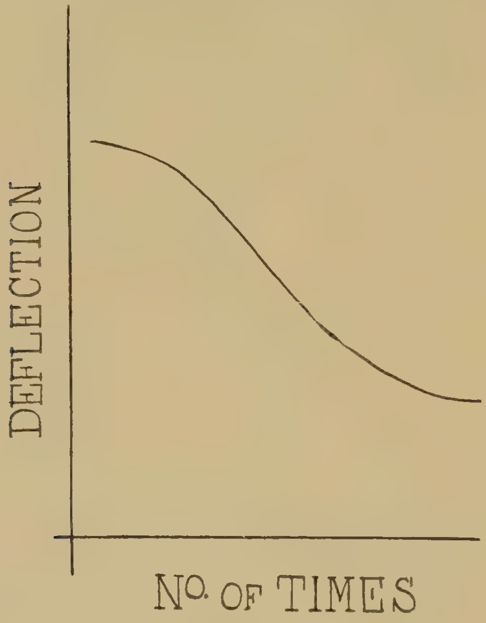

Fig. 7A.

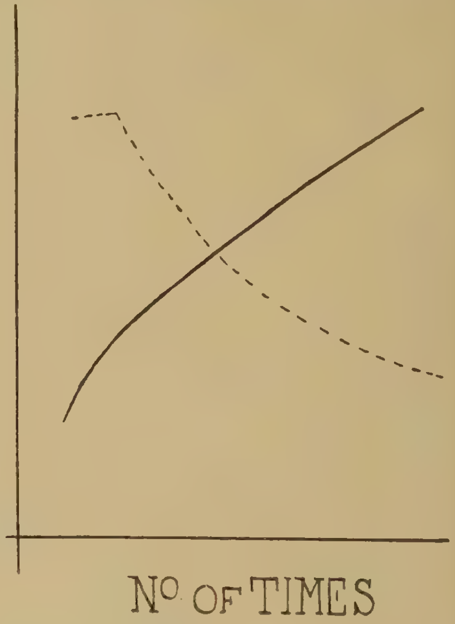

Fig. 8A.

IX. To show that Charge is Independent of Area for a Given Load.

Two specimens were taken in the sandwich form, and a certain load applied. The deflections obtained are shown. Weight 3,000 grms. (150 cm. to the volt). Temp. 19°C.

Load On.	*Load Off.*	*Load On.*	*Load Off.*
E. 5·2 L.	E. 5·2 L.	E. 5·2 L.	E. 5·2 L.
B. 2·9 L.	B. 1·1 L.	B. 2·7 L.	B. 1·1 L.
0·3 L.	3·5 L.	0·4 L.	3·6 L.
2·6 Right.	2·4 Left.	2·3 Right.	2·5 Left.

Each specimen was now cut in two and each half used to form two such sandwiches having the same faces together as previously. The same load applied gave deflections agreeing very closely with both sandwich forms. For one of the sandwiches, the following were the results obtained :—

Load On.	*Load Off.*	*Load On.*	*Load Off.*
E. 5·2 L.	E. 5·2 L.	E. 5·2 L.	E. 5·2 L.
B. 6·8 L.	B. 5·6 L.	B. 5·2 L.	B. 5·3 L.
4·4 L.	8·1 L.	3·2 L.	7·5 L.
2·4 Right.	2·5 Left.	2·0 Right.	2·2 Left.

Again the specimens were halved, and each sandwich treated in the same manner. The results below show a set of the readings obtained :—

Load On.	*Load Off.*	*Load On.*	*Load Off.*
E. 5·2 L.	E. 5·2 L.	E. 5·2 L.	E. 5·2 L.
B. 5·2 L.	B. 5·1 L.	B. 5·3 L.	B. 5·1 L.
3·2 L.	7·6 L.	3·1 L.	7·3 L.
2·0 Right.	2·5 Left.	2·2 Right.	2·2 Left.

The other sandwich gave similar results.

This indicates that for an unfatigued specimen the charge is proportional to the pressure. Halving the specimen doubles the pressure for a given applied force and the deflection is the same. This agrees with Curie's Law : " Charge is independent of the cross-section for a given total force."

X.

The other dielectrics examined in a similar manner were Glass, Hornoid, Sealing Wax, Rubber, Celluloid and Hard Paraffin. Space does not permit a full discussion of these substances, but in all cases the effect was quite definite. Some specimens, especially rubber, show the effect to an extraordinary degree. In general the remarks applied to ebonite will cover the phenomena observed with the other dielectrics.

A remarkable effect was observed with rubber when investigating the fatigue effect. The deflections might increase at first and then decrease, only to increase again with repeated applications of the load. A tendency was also observed to change sign on loading with this substance. Commencing with a deflection to the left, on loading, this decreased to zero and then became right on loading, left on unloading. A sudden reversal of sign was also frequently observed.

Hornoid and celluloid, in some arrangements of the sandwich form, appeared to become a conductor under stress, or removal of the stress, the initial large deflection on loading or unloading rapidly leaking away. This could not be traced to surface effect, and on plotting the initial throws curves of the typical kind were obtained, but the final readings were very minute and irregular.

The fatigue effect with hard paraffin was very pronounced.

XI. CONCLUSIONS.

The evidence is conclusive that all the dielectrics examined show the effect. Unfatigued specimens with small loads follow "Hooke's Law" for the charge is proportional to load. With larger loads there is fatigue and the corresponding deviation.

A. Mcl. Nicolson,* in his work on Rochelle salt, observed a similar effect. Some of his curves showed hysteresis or fatigue near the saturation values of the potential. The curves obtained with the dielectrics are similar to those obtained by him, and the specimens examined in this Paper indicate that, in general, they charge in a similar manner to Rochelle salt under pressure. That is, the opposite ends on which pressure is applied charge with the same kind and all other faces at right angles to this direction charge with the opposite kind.

The experiments on cubes show decisively that properties of the dielectrics (so far as this phenomenon is concerned) are different in different directions. There is difficulty in getting direct magnitude of the charges developed on the pressure faces (5) and (6). Possibly an estimate may be obtained indirectly from the results when the cube is pressed ⊥ (5 and 6), ⊥ (1 and 3), and ⊥ (2 and 4). Knowing Young's Modulus and Poisson's Ratio for the substance (these have been determined with care for ebonite), then assuming that the charge $\propto$ pressure $\propto$ contraction or extension, and assuming also homogenuity of structure, it should be possible to deduce the charges on faces 5 and 6. The experimental results indicate, however, that faces 1, 2, 3, 4 do not charge equally, not even always of the same sign ; and, furthermore, it seems unlikely that the elastic properties of, say, ebonite or rubber, would be the same in the planes parallel to rolled surfaces as in planes perpendicular to these surfaces.

It will be evident from the survey of the results given in this Paper that charges developed on the specimens are peculiarly susceptible to previous conditions of loading and treatment. Marked differences are also shown in the behaviour of specimens cut from the same piece.

XII.

It is very interesting to compare the magnitude of the charges developed in these experiments with those found by Curie and others in the examination of piezo-electric effects with crystals. In many of the latter experiments the specimen was insulated by ebonite blocks to which pressure was applied and the charges developed on crystal ebonite interfaces were measured. (In the work of P. Curie* on tourmaline he used glass as an insulator, Fig. 9.) Thus, for example, Curie† found that with a certain tourmaline specimen a load of 1 kilogram gave a charge of 0·053 E.S.U. Quartz gave a corresponding charge of 0·062 E.S.U. With the specimens of ebonite (Fig. 4) the charge developed on the first application of the load is 0·151 E.S.U. ; double that obtained by Curie for tourmaline. These are extraordinary specimens, and in general the charge developed is of the order 0·0185 E.S.U. for a load of 1 kilogram.

With a cube of the dielectric, the charge being taken from the vertical faces, the order of the charge is from 0·005 to 0·001 E.S.U.

The question then arises as to what really is measured in experiments on crystals where the charges on the loading faces are determined. Since there is so much variability in different specimens of ebonite, and in the same specimen at different times, it seems very advisable to repeat the experiments on crystals, using different insulators, with a view to seeing whether the charges developed for a given load are constant. Such experiments are being carried out at present and should furnish some interesting results.

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* Proc. Inst. of Elect. Eng. (American), p. 1325, Nov. (1919).

† Œuvres de P. Curie. Page 16, §1.

XIII.

Lord Kelvin* devised what he calls a piezo-electric pile, which served as a working model indicating the behaviour of a crystal under stress. This consists of copper and zinc sheets soldered together in pairs, each pair being insulated from the next pair by pieces of rubber at the corners. The top and bottom plates are connected to the quadrants of an electrometer (Fig. 10).

On allowing a weight to fall a short distance on to the top plate a big deflection of the electrometer is obtained. This is apparently attributed to the contact E.M.F. of the pile, due to the approach of the plates under pressure. From the work on rubber, which invariably gave a large deflection of the electrometer when a weight was "bumped" on the specimen, it appears that there might be another interpretation of the above. The rubber supports would charge up and the deflection, one anticipates, could be obtained if single sheets of copper were used.

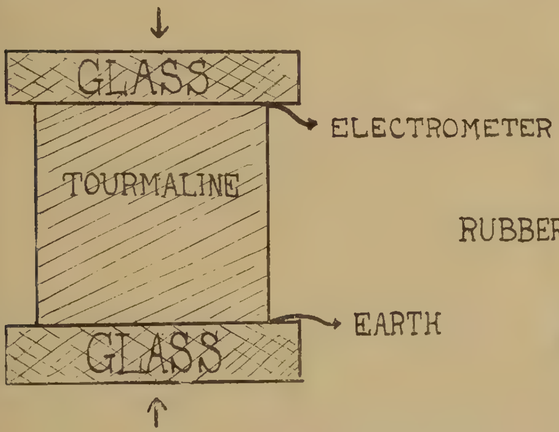

FIG. 9.

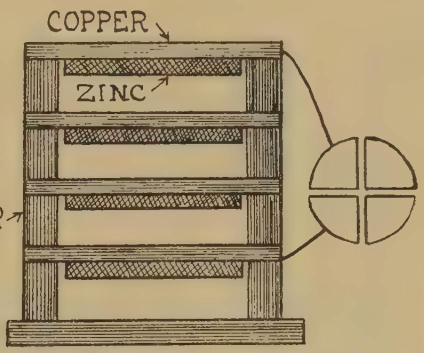

FIG. 10.

Four copper plates (6 cm. square) are used and separated by blocks of rubber (1×1×0·4 cm. approx.). On allowing a weight (690 grms.) to fall 2 mm. on to the upper plate there is a charge causing the spot of light to go wide of the scale and remain in that position.

Indeed, so sensitive is this apparatus to pressure that on slightly pressing the upper plate with a glass rod the spot of light goes off to the left, and on removing the pressure it goes to the right and remains wide of the scale. With zinc sheets the same effect is observed. When the zinc and copper plates are soldered together the falling of the weight on the upper plate still causes the spot of light to go and remain off the scale to the left. The weight was released by burning the thread supporting it. With blocks of 1 cm. cube, approximately, the zinc and the copper each produce the same effect, deflections are equal but smaller than in the above case, but when soldered together the effect was not so marked as when separate.

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* Coll. Papers, Vol. 5, p. 323.

XIV.

The similarity of the phenomena exhibited by the dielectrics examined in this research to those shown by crystals leads to the conclusion that the dielectrics possess what we may call " quasi-crystalline " characteristics. The manner of production of ebonite, rubber, plate glass, by rolling under pressure, would conceivably impart directive properties to the material giving it a quasi-crystalline nature.

Alternatively, it could be supposed that the material contained aggregations of crystals distributed more or less indifferently, and that these, when subjected to pressure gave rise to the effect.

The variability from specimen to specimen would be due to the difference in these aggregations, while fatigue and hysteresis effects would be due to the breaking down of these aggregations under strain. In fact, the material would possess a " fugitive " elasticity.

Sir William Bragg has been kind enough to have X-ray photographs taken of specimens of rubber and ebonite, with a view to determining whether there is any crystal structure in such materials. The photographs indicate that some such structure exists, but it is not possible to determine from them the nature and extent of the structure. In their general nature they support the suggestions which have been put forward in the preceding pages.

The preceding research has been carried out in the Physics Department of Battersea Polytechnic. My best thanks are due to Dr. Marsh for suggesting the subject and for the valuable criticisms and information that he gave when discussing the results.

DISCUSSION.

Dr. R. T. BEATTY, in congratulating the author, said that there is no doubt that many dielectrics show the effect very strongly, a circumstance which causes much inconvenience. It is important to find substances which are free from piezo-electric properties, and as a matter of fact samples of ebonite can be found with only one-thousandth of the sensibility of quartz. Holohedral crystals such as calc-spar, and quartz compressed parallel to the optic axis, are also free from the effect. In the case of quartz an absolute test has been made by forming a condenser out of a plate of this crystal sandwiched between tin foil and held between knife edges. This condenser can vibrate mechanically with a definite frequency, so that when it is included in the tuned circuit of an oscillating valve it exerts in the general case a marked effect on the oscillating potential when the mechanical and electrical frequencies are in resonance. When, however, the crystal is cut perpendicular to the optic axis no effect can be detected, although one ten-thousandth of the usual effect would be observable. The speaker expressed the hope that the author would try to find a cheap and manageable dielectric free from piezo-electricity.

Dr. A. O. RANKINE said that when he heard of the contents of the Paper not long before the meeting he was reminded of an old experiment shown by Dr. Porter, but as the voltages involved were of very different orders he had since concluded that the phenomenon in question was not related to piezo-electricity. He then demonstrated that if two strips of celluloid be firmly held together at one end with one hand and bent upwards with the fingers of the other hand, and if the hands be then moved apart so as to make the bend in the strips travel from one end to the other, the inner or contacting surfaces of the strips become electrified, the convex inner surface being positive. He had at first attributed this result to piezo-electricity, but was now inclined to think it due to friction combined with action of the electrophorus type arising from imperfect insulation from the earthed body of the experimenter.

Mr. F. E. SMITH questioned whether the effect investigated by the author could be identified with the effect observed by Curie, which was only exhibited by crystals. Ebonite is a substance of very uncertain composition ; but if Rochelle salt, for instance, were powdered and mixed with rubber, no piezo-electric effect should be expected, because the optic axes of the crystals would point in all directions at random. On the other hand, most insulators become electrified under pressure, as is the case with glass when mercury falls into a beaker, for instance. Curie was unable to obtain his effect with some crystals, and Prof. Nicholson, who had recently sent the speaker crystals for use in a piezo-electric telephone, had been obliged to grow these with great care, not all the crystals obtained being successful. It was to be hoped that the author would find time to repeat Curie's experiments, and would be able to settle the question as to the relatedness or otherwise of the phenomena.

Dr. S. MARSH said that Voigt, who had been the first to treat the piezo-electric effect mathematically, had found variations of 5 per cent. or 10 per cent. in the charges obtained from crystals under similar conditions, and had supposed these differences to be due to variations in crystals coming from different places of origin. More probably, however (and particularly in the case of sugar crystals), the errors were due to electrification of the blocks of insulation. Electrification by friction could perhaps be better interpreted as electrification by pressure, and experiments carried out in America in connection with the charging of machine belts could be explained on this theory no less than on the theory of contact potential which had been put forward. With regard to the experiment shown by Dr. Rankine, experiments, which have been in progress for some time, on the electrification of dielectrics by bending, indicated charges of the same order of magnitude as those obtained by the Author, The signs of the charges obtained agreed with those to be expected from the Paper, since the convex strip would be in tension and vice versa.

Dr. E. A. OWEN said that in the case of metals, rolling produces a definite orientation of the crystals, and, after rolling, some of the crystal planes shown by X-ray analysis are found to be suppressed. Since ebonite is rolled, a similar orientation is no doubt produced amongst the crystals it contains.

Dr. F. LL. HOPWOOD suggested that a true test for the piezo-electric effect would be afforded by trying to produce the inverse effect, viz., by charging the dielectric and observing the resulting strain.

AUTHOR'S reply (communicated): I would like to thank Dr. Beatty for his kind remarks. With regard to the directive properties of quartz—the piezo-electric effect being absent when cut perpendicular to the optic axis—it is unfortunate that a similar effect is not found in dielectrics. As has been pointed out in the Paper the magnitude of the effect depends very largely on the previous treatment of the specimen under consideration, and it appears that if the specimen were subjected to considerable stress for some time, in order to break down these aggregations of crystals, or a means were sought to eliminate the possible arrangement of these crystals, then the specimen would not show the effect.

Dr. Rankine's interesting demonstration of Dr. Porter's experiment could be explained on this principle, and at present experiments are being carried out by a colleague of Dr. Marsh, whereby the charge is developed by bending the dielectric and not by subjecting it to a pressure. Further explanations of this experiment should therefore be forthcoming.

Dr. Owen has given evidence to support the theory that directive properties might be imparted to the dielectric in the course of its manufacture, and if the powdered Rochelle salt mentioned by Mr. F. E. Smith were mixed with rubber under similar conditions—so that a definite orientation was given to them—then the mixture should show the effect.

That this is an internal crystal effect is emphasised by the fact that if a sheet of rubber be left for a long period so that the surface deteriorates then the effect is absent with such specimens. On cutting the rubber surfaces away to expose fresh rubber the effect is again very pronounced.

The inverse effect has been established for glass by Quincke, and Lippmann has also dealt with this subject. To reconcile these readings with the specimens used in these experiments presents great difficulty. In the first place it is impossible to keep the "state" of the dielectric constant, and with such substances as ebonite the experimental observations of the strain produced by charging the substance are difficult to obtain.

The other important points were dealt with by Dr. Marsh in his statement.

RECENT WORK IN STELLAR PHYSICS.

By E. A. Milne, *M.A., Fellow of Trinity College, Cambridge, and Assistant Director of the Solar Physics Observatory, Cambridge.*

(A Lecture delivered before the Society on *January* 25, 1924.)

THE existence of two great classes of stars, the giants and dwarfs, was discovered by Hertzsprung(1) in 1906. But it was in 1913 that this became more than an impressive fact. In that year, at a meeting of the Royal Astronomical Society, Professor H. N. Russell put forward his considered theory of giant and dwarf stars.(2) In the same year, in the Philosophical Magazine, Professor Niels Bohr put forward his theory of spectra.(3) Both theories made vivid appeals to the imagination, and both theories have become, at the hands of their authors and others, powerful weapons of elucidation. At a first glance, the two theories dealt with widely different material. The theory of Bohr dealt with the atom, and it rested at that time on a few clear-cut facts and precise measurements of wave-length, combined with the fruits of general electrodynamic and thermodynamic theory. The theory of Russell dealt with the stars, and depended on an array of statistics consisting for the most part of ill-determined quantities such as their masses, densities, surface brightnesses and parallaxes. But both theories were concerned with spectra.

The great advance which Russell made was his systematic correlation of the stellar quantities just mentioned with the observed stellar spectra, that is to say, with a purely descriptive variable, the spectral type. The X-ray spectra of the elements can be arranged in linear order, the order of the atomic numbers. The optical spectra of the elements show no such simple linear relation to one another. But the spectra of the stars do show a linear order. There is a relation of " betweenness " amongst them. The overwhelming majority of stellar spectra can be arranged in a sequence, with a continuous gradation from one end to another. Viewed in relation to the known complexities of optical spectra this is remarkable. It renders incredible the earlier view that differences in stellar spectra arose from differences in chemical composition. The type of spectrum appears to be on the whole a function of a single variable, and we are compelled to assume that the spectrum appearing depends on the variation of one of the quantities which describe the physical conditions in the stellar atmosphere. Two main variables suggest themselves—the temperature and the pressure.

But neither of these are capable of direct measurement. And it is even more absurd to speak of the temperature and pressure of a star's atmosphere, without further precision of expression, than it would be to speak of the temperature and pressure of the earth's atmosphere. The pressure and temperature in the earth's atmosphere change conspicuously from level to level, and so they will in a star's atmosphere ; but we can at least speak of the mean pressure and mean temperature of the earth's atmosphere. In the case of a gaseous star—and roughly speaking all stars may be taken to be gaseous stars—who is to say where the atmosphere leaves off and the star begins ?

Direct determination of the physical conditions in a stellar atmosphere thus eludes us. There is, however, one variable capable of direct measurement, namely, the "effective temperature" of the star. Unlike the pressure and the actual temperature in an atmosphere, the amount and spectral composition of the radiation per unit area leaving the surface of the star—that is, passing outwards through the higher levels of the star's atmosphere—are definite. They may be compared respectively with the amount and spectral composition of radiation in thermodynamic equilibrium at a given temperature. From either kind of comparison we may determine the equivalent black body temperature ; this temperature, which is merely a convenient co-ordinate, is called the effective temperature of the star. We know that for the sun the effective temperature as determined from the amount of radiation is in fair agreement with the effective temperature as determined from the intensity distribution in the continuous spectrum. It is plausible to assume that the actual temperature in the stellar atmosphere will be intimately connected with the effective temperature.

The variation from star to star in spectral composition of the emitted light is obvious to the eye as a variation in colour. Red stars will have the lower effective temperatures, white and whitish-blue ones the higher ones. But the classification into colours coincides, in great measure, with the classification into spectral types from the absorption lines, and measurements of effective temperature confirm the correlation. The single variable, of which the descriptive spectral type is principally a function, is thus the effective temperature.

Russell's correlations of luminosities and densities with spectral types were, therefore, reducible to correlations with effective temperature. In so far as effective temperature is a measure of the amount of radiation per unit area, it is also a measure of surface brightness, and hence they were also correlations with surface brightness. And it was by way of surface brightness that the observed facts were brought into harmony. An absolutely very bright star of the same spectral type as an absolutely very faint star will have approximately the same surface brightness, and must, therefore, have a larger surface. But the masses of the stars being much the same, the density of the bright giant star must be smaller than the density of the faint dwarf star—up to a million times smaller. We have here, in a couple of sentences, the interpretation of the observed division into giants and dwarfs.

In this formulation, the descriptive spectral type has dropped out. But it should be remembered that the statistics collected by Russell and others referred to the observed spectral types of the stars ; the evidence concerning the relation of type to effective temperature was definite but comparatively meagre. Moreover the connection between spectral type and effective temperature was left almost wholly unexplained. It was known that some spectral lines could be produced, in the laboratory, only at high temperatures or under intense discharges, and that such lines were often only to be found in stars with high effective temperatures. But it was not known why this was so, or why the same lines tended to disappear at still higher temperatures ; and of quantitative explanation there was none. There appeared to be a definite relation between effective temperature and type of spectrum, but the connection was empirical. There was a gap in the logical argument.

Bohr's theory of spectra has made it possible to bridge over this gap in the theory of the stars. It has become possible to use the theory of spectra to deduce from the observed stellar spectra something concerning the true temperatures of the various levels in stellar atmospheres, and to ascertain that the true temperatures are in general agreement with the effective temperatures. We now see that the observed spectra are precisely those which ought to appear at the different effective temperatures; even if all the elements are present in a stellar atmosphere, the Bohr theory shows that only some of them can signify their presence by their line absorption spectra, at any given temperature, and it predicts the successive appearance and disappearance of lines with increasing temperature.

It also accounts for those less conspicuous but highly significant spectral variations which are known to cut across the linear order of the spectral types, and which distinguish, inside each type, not only the giants from the dwarfs, but the brighter giants from the fainter giants and the brighter dwarfs from the fainter dwarfs. We saw that the main variable governing the spectral sequence was temperature, though we did not forget that another possible variable was the pressure. Now that we know that the spectra inside a given division of the main sequence show evidence of a cross variation, we might conjecture that the pressure would be the new variable. Developments of the Bohr theory show why this is so.

These fundamental applications of the Bohr theory of spectra to stellar atmospheres will always be associated with the name of Dr. Megh Nad Saha.(4)

I will endeavour to review some of the recent additions to our direct knowledge of stellar spectra and stellar temperatures, and then show how both these and the wealth of earlier observations on stellar spectra can be accounted for, in a simple manner, in the light of modern atomic theory.

As regards spectra, I will mention only the subject of spectroscopic parallaxes. So early as 1913, Adams and his collaborators at Mount Wilson found that amongst stars of the same spectral type, those of small proper motion were relatively fainter in the violet than stars of large proper motion.(5) Statistically, small proper motion means large distance, and therefore for given apparent magnitude, great absolute brightness. Thus giants appeared to have spectra fainter in the violet than dwarfs of the same type. This was one of the early indications of the now well-established fact that for given spectral type, giants have lower effective temperatures than dwarfs. Extending his observations to line spectra, Adams found that inside a given type, certain lines increased in intensity with increasing absolute luminosity, whilst other lines decreased. Enhanced lines like λ 4216 Sr^+, were most intense in the absolutely brightest stars; "low temperature" lines, like λ 4607 Sr, were most intense in the absolutely faintest stars. By taking stars of known absolute magnitude, he was able to construct empirical curves connecting intensity with luminosity for different lines, and then to use the curves to deduce the luminosities of other stars by estimation of the line intensities in their spectra. Comparison of the absolute magnitudes thus found with the apparent magnitudes gave the distance. In this way the distances of hundreds of stars have been found from their spectra. The basis of the determination is empirical, but any theory of stellar spectra must account for the correlation, inside a spectral type, of line intensity with absolute luminosity, and for the differing behaviour in this respect of different lines.

An idea of the distribution of light in the continuous spectrum of a star can be obtained from its colour index, which is defined as the excess of its photographic apparent magnitude over its visual apparent magnitude. The redder the star, the weaker it is relatively in photographic radiation for a given intensity of visual radiation. The two magnitudes, roughly speaking, are measures of the intensity in two different regions of the spectrum (e.g., $\lambda 4250$ and $\lambda 5700$), and assuming the intensity curve follows Planck's law, the effective temperature can be deduced from the observed intensity ratio in these two wave-lengths. The following are some colour-indices recently published from Mount Wilson.(6)

Spectrum.	Giants.		Dwarfs.	
	C.I.	T.	C.I.	T.
Bo	−0·32	10,500°	...	...
B5	−0·17	10,000	...	...
Ao	0·00	9,230	...	...
Fo	0·38	7,000	...	...
Go	0·86	5,300	0·72	5,770
G5	1·15	4,610	0·83	5,500
Ko	1·48	3,860	0·99	4,880
K5	1·84	3,270	1·26	4,120
Ma	1·88	3,080	1·76	3,330

The larger colour-indices, and hence lower effective temperatures of the giants as compared with dwarfs of the same type are well shown. (It should be mentioned however that the temperatures in this table have not been deduced directly from the colour-indices, but have been taken from Wilsing's temperature determinations for giants, which are probably much too low for the hotter stars.)

Another quantity which has been largely measured in recent years is the effective wave-length.(7) When a coarse diffraction grating is placed over the objective of a telescope, each star photographed gives a central image together with a series of fainter lateral images which are in reality minute spectra. The centre of gravity of each lateral image depends on the distribution of intensity in the light of the star, and measurement of the distance between say the first pair of lateral images gives the effective wave-length for the particular optical and photographic equipment used. Effective wave-lengths decrease as the spectral type becomes earlier, and are longer for giants than for dwarfs of the same type.

Investigations neither of colour-index nor of effective wave-length can be considered as satisfactory substitutes for detailed investigations of the distribution of intensity throughout the spectrum. The visual spectro-photometric observations of Wilsing, Scheiner and Münch,(8) and the photographic spectro-photometric observations of Rosenberg,(9) each made at a series of wave-lengths throughout the accessible spectrum, are investigations of this kind. They have recently been re-analysed by Brill,(10) and extracts from his reductions are shown in the table below.

Spectrum.	Wilsing.	Rosenberg.	Spectrum.	Wilsing.	Rosenberg.
Bo	12,300°	30,000°	Go	5,980	6,000°
B5	11,480	18,000	G5	5,280	5,300
Ao	10,250	12,000	Ko	4,570	4,570
A5	9,000	9,000	K5	4,000	3,840
Fo	7,950	7,850	Ma	3,500	3,580
F5	6,880	6,930			

Different reductions give different scales, especially for the early-type stars, but the Rosenberg temperatures are invariably considerably higher than the Wilsing ones above type Ao. It is generally admitted that the Wilsing observations give too low temperatures for the B-type stars, and the true values probably lie between the two scales.

By the use of a photo-electric cell for measuring the intensity distribution in stellar spectrograms some highly interesting results have been obtained by Professor Sampson.(11) Some of his temperatures, obtained by comparison with Capella (5,500°), are given below :—

γ Cass.	Bo	16,900°
α Lyrae	Ao	11,200
α Aquilae	A5	10,000
β Cass.	F5	8,800
Polaris	F8	6,800
ϵ Cygni	Ko	5,100
α Cass.	Ko	4,700
β And.	Ma	3,500

Some determinations of stellar temperatures by the wedge method of spectrophotometry have just been published by H. H. Plaskett.(12) They are as follows :—

γ Cass.	Bo	15,000°
ϵ Persei	Bo	15,000
α Cygni	A2	9,000
δ Cass.	A5	9,000
α Aurigae	Go	5,500-6,000
β Gem.	Ko	5,000-5,500

He has also investigated the sun's continuous spectrum, using a fine slit, and has found it to approach that of a black body much more nearly than was previously supposed.

Remarkably successful attempts have been made during the last few years in the direct measurement of the heat of the stars. A form of non-selective receiver, consisting of a thermo-couple mounted in a vacuum chamber fitted with a fluorite window, has been developed by Coblentz. It is some hundreds of times more sensitive than any of the stellar radiometers previously designed ; galvanometer deflections of 15 cm. can be obtained for a bright red star like α Orionis, and a deflection of 1 mm. even for fourth magnitude stars of type B. The first measures on stars were made at the Lick Observatory(13) in 1914, and recently a great number of observations have been made at the Lowell Observatory(14) and at Mount Wilson.(15) By the use of suitable transmission screens the energy distribution throughout the spectrum can be obtained, and this in direct energy units, without the calibration required in photometric observations. The following are some of the temperatures deduced by Coblentz from his observations.

Temperatures from Thermo-couple Observations.

ε Orionis	Bo	13,000°—14,000°
β Orionis	B8p	10,000 —12,000
α Can. Maj.	Ao	8,000 —11,000
α Can. Min.	F5	5,500 — 7,500
α Aurigae	Go	5,300 — 6,500
α Boötis	Ko	3,500 — 4,500
α Tauri	K5	2,800 — 4,500
α Orionis	Ma	2,800 — 3,300

Both in spectral distribution observations and in variable star observations the stellar radiometer is likely to have a great future.

The recent measurement of the angular diameters of stars at Mount Wilson, by means of a form of interferometer designed by Michelson, has attracted universal attention.(16) From the point of view of the present lecture, the importance of the achievement is that it affords a direct measure of surface brightness, and so of effective temperature. For, according to the well-known laws, the radiation received from a luminous object is related to its absolute surface brightness by the formula :—

Radiation received = (Surface brightness) × (Solid angle subtended at observer).

For a star, this becomes

$$\text{Surface brightness} = \frac{\text{Apparent brightness}}{(\text{Angular diameter})^2}$$

The magnitude of a star gives its apparent brightness. The constant of proportionality may be found either from a knowledge of the candle-power equivalent of the magnitude scale, or by comparison with a star of known angular diameter and surface brightness like the sun. The measured angular diameter of α Orionis, 0·047″, gives in this way a surface brightness of 4·6 magnitudes compared with the sun's surface brightness as zero. This yields a temperature of 3,000°, in good agreement with its type Ma.(17) Similarly α Boötis (Ko) has an angular diameter of 0·022″, which gives a surface brightness of 2·2 magnitudes and a temperature of 4,100°.

These diverse lines of investigation agree in finding spectral type to be mainly a function of effective temperature. They are in agreement as to the order of magnitude of the temperatures, and as to the numerical values for the late-type stars. They differ somewhat for the early-type stars.

They agree further, when attention is paid to finer details, in finding effective temperature to be a function of absolute magnitude as well as spectral type. As regards the continuous spectrum, for given spectral type, the greater the absolute luminosity the lower the effective temperature ; but as regards the line spectrum, the greater the absolute luminosity the greater the development of enhanced lines. This is paradoxical. Inside a main spectral class—that is to say, out of a group of stars having the same general type of spectrum—the giants have lower effective temperatures, but greater development of what used to be called high-temperature lines.

Any determination of effective temperature from the relative intensities in two parts of the continuous spectrum is obtained by applying Planck's formula. Different pairs of places will not yield the same temperature unless the continuous spectrum is that of black radiation. Roughly speaking, the continuous spectra of the stars agree with the spectrum of black radiation, but it is not known with what precision. Clearly the question is complicated by the absorption lines. It is necessary to measure the intensity between the lines, but this may be difficult when the lines are close together. To some extent, too, the presence of absorption lines may be expected to alter the distribution of energy in the continuous spectrum. For the layer of gas causing an absorption line cannot be supposed to store up the radiation it absorbs. In the steady state it will emit it, both outwards and inwards ; the outward emission is the residual intensity we observe in the line ; the inward emission is returned to the star, and tends to keep it a little hotter than it otherwise would be. Thus owing to the absorption lines the distribution of intensity in the continuous spectrum is likely to correspond to a higher temperature than would be given by the total amount of radiation. We can see from a special case that the presence of absorption lines is likely to have some effect on the continuous spectrum. For if we imagined the spectrum to become so crowded with lines that no continuous spectrum was left, we should have the same state of affairs as if there were no absorption lines at all. The total amount of energy leaving the surface of a star probably depends almost entirely on what is happening in the interior of the star and very little on the structure near the surface, and it must break through in some part of the spectrum.

Another possible source of departure from the black body spectrum lies in the temperature gradients in the outer layers. The radiation leaving the surface will consist of the superposition of pencils of radiation emitted at different depths, each weakened by absorption in the layers it traverses. For each wave-length there will be a certain average depth into which we see, depending on the transparency in that wave-length ; the radiation leaving the surface may be considered to have originated at this average depth. If the average depth is much the same for all parts of the spectrum, the radiation in the different parts of the spectrum will correspond to the same temperature, namely, the temperature at the level of the average depth, and the resulting continuous spectrum will approximate to that of a black body. But if the transparency is different in different regions, the radiation will emerge from different average depths ; where the transparency is larger than the average, the radiation will come from a greater depth, i.e., a higher temperature, and will be more intense. The resulting continuous spectrum will not be that of a black body.

Again, gaseous scattering in the stellar atmosphere may distort the spectrum. For gaseous scattering varies inversely as the fourth power of the wave-length, and so would depress the intensity in the blue and ultra-violet regions. Whether or no scattering in a stellar atmosphere is appreciable depends simply on the relative size of the coefficients of general absorption and of scattering. If absorption is relatively more important, then as we go outwards from the interior we shall reach a level where the density is too small to cause any appreciable scattering before we have got free from the region of appreciable absorption and emission. There is some evidence that this is so in the case of the sun.*(18)

We have mentioned some of the circumstances which might cause a departure from blackness. It may be objected that the onus of proof is the other way ; why should the continuous spectrum be expected to approximate in the least to that of black radiation ? The answer is that theory indicates that any sufficiently extensive mass of material, even a gas, at a uniform temperature should give a black body spectrum apart from scattering ; we are only called on to examine the consequences of temperature gradients. Only to the extent to which a beam of external radiation incident on the star from outside would be partially and not completely absorbed would the spectrum depart from black body, in the absence of temperature gradients.

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* Scattering by free electrons has been discussed recently by several writers. Compton (Phil. Mag., Nov., 1923) has appealed to his quantum theory of scattering by free electrons to account for the slight shift of lines to the red between the centre and limb of the sun, since a free electron *at rest* scatters a quantum of smaller frequency and there should be more scattering at the limb than at the centre. The electrons in the solar atmosphere, however, will have an approximately Maxwellian velocity distribution, and the recent work of Pauli (Zeit. für Phys., 18, 272, Oct., 1923) shows that the spectrum of black radiation is unaltered by interaction with free electrons having a Maxwellian velocity distribution of the same temperature as the radiation. This shows that for such a distribution there is no systematic tendency for all quanta to become scattered as quanta of lower frequency, and therefore it is improbable that the centre-limb effect can be accounted for in Compton's way.

Thermodynamic considerations of this kind leave out of account, however, the *origin* of the continuous spectrum. The emission of radiant energy, according to atomic theory, is always monochromatic, and a continuous spectrum is only produced by the superposition of a number of quanta of different frequencies. As an example of this, we have the continuous spectrum which exists beyond the limit of an optical series. As an emission spectrum, it is produced by the capture of an electron by an ionised atom ; the frequency emitted depends on the initial kinetic energy of the electron as well as on the ionisation potential of the atom for the energy level concerned. As an absorption spectrum, it is produced by the photo-electric emission of an electron, with consequent ionisation of the atom. The continuous spectrum which originates in this way beyond the limit of the Balmer series is known in celestial spectra ; in emission it is known in the sun's flash spectrum ; in absorption it is known in some of the hotter stars.(19) In the interior of a star almost the whole of the radiation has been attributed by Professor Eddington to such photo-electric ionisations and captures.(20) It is not yet possible to say whether the continuous spectrum of the visible region is made up, in this way, by the superposition of a number of pieces of continuous spectrum. The lowest optical levels of atoms lie rather far in the ultra-violet, and we cannot expect captures into the outer levels to be sufficiently frequent. On the other hand, a good deal of radiation may be emitted by electrons which fail to be captured but jump, during encounter, from one hyperbolic orbit to another.

It has been suggested by Lindemann and others that the Stark effect may be the origin. Where the atoms begin to be more tightly packed, in the deeper layers, they will exert on one another considerable electric fields of a random character. This will cause fluctuations in the energy levels, and consequent broadening of lines ; and by the superposition of multitudes of broadened line-emissions a continuous spectrum originates. The phenomenon is the same as that of the broadening of lines under pressure. But it must be mentioned that faint lines in the solar spectrum, some of which probably originate at considerable depths, are often very fine.

It is clear that if the star's continuous spectrum is approximately that of a black body the true temperature of the average depth from which the issuing radiation originates must be closely the same as the effective temperature. To go further than this we require to know something of the temperature gradients in the layers yielding the radiation. Assuming the outer parts of the star are in a steady state, the energy radiated to space must be transported to the surface region from the deeper regions. It was recognised long ago that convection is inadequate. A possible state is one in which all the interchanges of energy are radiative ; the deeper regions radiate to the surface regions, the surface regions in turn to space. The balance of outward over inward radiation across any level is constant, and equal to the radiation to space from the surface. In this state of affairs each level absorbs as much as it radiates ; it absorbs energy both from the outward beam from the layers beneath it and from the back radiation from the layers above it ; it radiates equally, however, both outwards and inwards, whilst the energy it absorbs is derived principally from the much more intense outward beam. Consequently, for a steady state, there must be a gradient in the radiation intensity, positive inwards, with a consequent gradient in the temperature. The gradient is very simply related to the net radiation, and as Schwarzschild first showed, the actual temperature of the surface layer should be some 20 per cent. lower than the effective temperature as judged from the total amount of radiation.(21) We thus deduce an actual temperature from the mere co-ordinate temperature. Further, if the opacity is the same for all wave-lengths, the spectral composition of the radiation should correspond to a temperature some three per cent. higher than the effective temperature.(22)

The disc of the sun is darker toward the limb than at the centre, and it is probable that the same is true for the stars in general. The darkening is merely a consequence of the temperature gradient ; near the limb, the tangentially emerging radiation has necessarily originated at smaller depths where the temperature is lower, and is hence less intense. From the observed distribution of intensity over the sun, we can deduce the temperature gradient in the layers yielding the continuous spectrum.(23) It is found to agree closely with that calculated on the hypothesis of radiative equilibrium. Limb-centre observations, therefore, confirm the deduction of the boundary temperature from the effective temperature on the radiative equilibrium hypothesis.

The line spectrum of a star originates in levels which are, on the whole, above the levels in which the continuous spectrum originates. The names " reversing layer " and " photosphere " are used for the two regions, but it should not be forgotten that each merges continuously into the other. Let us attempt to construct a picture of a typical stellar atmosphere.

It will consist of layers of gas, of differing pressure, density, temperature and composition. There will be few compounds—the temperature will be higher than their temperature of dissociation. For the same reason the gases will be almost entirely monatomic. Many of the atoms will be ionised, and there will be an abundance of free electrons ; some will have been liberated by collisions between atoms or atoms and electrons, but the majority will have been liberated photo-electrically At the lower levels, where the density is sufficient, free electrons will be constantly colliding with ionised atoms and recombining ; but at the higher levels collisions and recombinations will be rarer. Owing to the abundance of free electrons the gases will be highly conducting, and there will be no appreciable macroscopic electric fields

How much disturbance there will be in general we do not know. In isolated regions of the sun, in sunspots and prominences, the disturbances may be enormous but in general the disturbances are insufficient to set up a convective gradient There is, therefore, a tendency for the heavy atoms to settle out towards the bottom and for the light atoms to predominate at higher levels. We must except free electrons from the category of light atoms in this connection, since the electrostatic forces between them and their ions will prevent any appreciable separation—the gas as a whole in any small region will be electrically neutral.(24) But there is only a *tendency* for the atoms to settle out according to mass. For every time an atom absorbs a quantum of radiation $h\nu$, it also absorbs momentum of amount $h\nu/c$. I is true that it quickly re-emits the quantum and returns to a state of lower energy and that the atom will acquire the momentum of recoil. But emission will occur n a random direction, whilst absorption will be principally the absorption of outward moving radiation. Thus on the whole the atom will gain momentum in the outward direction. This pressure of radiation will tend to sustain the atoms against the pull of gravity, and will relieve the pressure gradient. It will act unequally on the different atoms. The higher the frequency the greater the associated momentum, and so the pressure of radiation on a given atom will depend on the wave-lengths it can absorb. It will depend, too, on the number of quanta that are available—that is, on the intensity of the continuous spectrum. It will depend further on the rapidity of the process of absorption and re-emission. If for simplicity we consider a hypothetical atom with only two levels, when once it has absorbed a quantum it is incapable of absorbing another until it has returned to its normal state; for the purposes of the pressure of radiation it is out of action until it re-emits. The fate of an atom which chances to become ionised is still worse. Except in so far as its spark spectrum provides it with suitable lines, it is incapable of responding to radiation pressure until it recaptures an electron. The time during which an atom remains in an excited state is thus of fundamental importance. These atomic circumstances gravely interfere with the ordinary tendency to settle out under the influence of gravity only, and so though there will be a statistical tendency for the heavier atoms to be at the lower levels, we are prepared for considerable exceptions, depending on the several atomic structures.

As we proceed outward from the photospheric layers we first encounter layers of low density in which the atoms begin to be free to absorb and to emit their line spectra without appreciable interference by other atoms. Each elementary layer of atoms returns half of what it absorbs, the other half being radiated to the layers above. These in turn absorb a fraction of the now attenuated radiation, and return half and pass on the other half. In this way the monochromatic absorption lines are formed. To the radiation of wave-lengths lying between the absorption lines the atoms are completely transparent, and it passes through them with little or no effect on their temperature. There will, however, be a certain amount of interchange between radiant energy and the energy of molecular motion, by means of inelastic electronic impacts and three-body encounters, which will exert some control over the temperature. We are now above the specific region of the Schwarzschild boundary temperature, but the gases will have approximately this temperature.

In the lower parts of the region we are discussing, the weight of the atoms will be largely supported by the pressure gradient; for, partly because many of the atoms will be in the higher quantum states, and partly for other reasons, there will be a considerable number of passenger atoms, which have to be supported and do not make much use of the pressure of radiation. As we ascend, the pressure will at first decrease exponentially, and the pressure gradient will decrease. The passenger atoms, and many of the heavy ones, will be left behind, and we shall reach a region of small pressure and small pressure gradient which will consist of only those atoms, so to speak, which can support themselves by their own resources. This is the chromosphere. Here collisions will be exceedingly rare, and each atom will be held against gravity chiefly by the succession of impulsive jerks arising from the absorption of quanta. The concept of temperature is hardly applicable to such a region. The atoms present will be those which have conveniently situated absorption lines, which after absorption tend to return to a state readily available for the next absorption, which do not too readily lose their functioning electron and which are not too heavy.

Let us now consider the type of absorption spectrum that will be produced by a stellar atmosphere of this kind. In the first place, the atoms in the high chromospheric levels, supported solely by radiation pressure, will be too few to have any appreciable effect on the formation of absorption lines. At a total eclipse of the sun they are visible as the flash spectrum, because, viewing them tangentially, we are looking at a considerable thickness. Viewed radially, however, they amount to very little. This is evidenced by the helium lines, which though prominent in the flash spectrum—where, in fact, helium was first discovered by Janssen and Lockyer—hardly appear in the ordinary Fraunhofer spectrum. The absorption lines are formed by the atoms in the levels where there is an appreciable pressure gradient, where the density is not too small and not too large, and where on the whole the atoms will be endowed with a Maxwellian distribution of velocities corresponding to a little less than the effective temperature of the star.

Now consider a line like the λ 4227 line of calcium. This is the first line of the principal series and is denoted by $1S-1P$. For a calcium atom to be capable of absorbing it, its outermost electron must be in the state of lowest energy—the $1S$ state. The act of absorption consists in the electron switching from this state to the $1P$ state. Hence the strength of the absorption line λ 4227 produced by the stellar atmosphere will depend on the number of Ca atoms there are in the reversing layer with a valency electron in the $1S$ state. But all the Ca atoms that are present will not be in this state. In the first place, some will have just absorbed the line, and be in the $1P$ state; others will be in other states, such as the $1D$ state, the $1p$ state, &c.—perhaps owing to absorption, perhaps owing to inelastic impact. But in the second place, some of the atoms will have lost one electron, either by collision or more probably photo-electrically. These atoms, too, will be out of action from the point of view of absorption of λ 4227, until they have recaptured an electron. But we must note an essential difference between the two ways of being out of action. How rapidly an atom with a displaced electron recovers its normal state depends on various things, but chiefly on the structure of the atom itself. But the rapidity with which an ionised atom regains its neutral state depends in addition on the chance of its encountering an electron. The excited atom still retains its partner, but the ionised atom has to find a new one, and the ease with which it does this depends on the number of partners available—that is to say, on the concentration of free electrons. Thus the proportion of atoms that are ionised depends in an intimate way on the pressure; the lower the pressure, or, more precisely, the lower the partial pressure of free electrons, the smaller the chances of re-combination, and thus in the steady state the bigger the proportion of ionised atoms. This dependence of high-temperature ionisation on pressure was first pointed out by Saha, and has been amply confirmed by experiment.

The ionised Ca atoms are out of action as regards the absorption or emission of λ 4227, but they can emit and absorb a spectrum of their own. The well-known H and K lines of the Fraunhofer spectrum are the first pair of the principal series of ionised calcium, and therefore the analogues, for the ionised atom, of λ 4227 for the neutral atom. They are originated by switches of the remaining valency electron.

As the temperature rises, the pressure remaining constant, the proportion of ionised atoms increases and that of neutral atoms decreases. Hence the intensity of λ 4227 as an absorption line should decrease, whilst that of the *H* and *K* lines should increase. This will go on until in turn second-stage ionisation sets in, with loss of the second electron. The spectrum of the calcium atom with two electrons lost will be far in the ultra-violet as regards its more intense lines, and for our purposes we may say that no fresh lines can appear.

This is exactly what is observed in the sequence of stellar spectra, as we proceed from the red *M*-stars with atmospheric temperatures of about 3,000°, to the white or blue *B*-stars and *O*-stars, with temperatures of 20,000° or more. The intensity of λ 4227 steadily decreases, disappearing completely about type Ao. That of $\lambda\lambda$ 3933, 3968 steadily increases up to type Ko, remains strongly intense in solar type stars, and thereafter steadily decreases, but remaining long after λ 4227 has disappeared.

The same holds good of the strontium singlet λ 4607 ($1S-1P$) and the enhanced doublet $\lambda\lambda$ 4216, 4078 ($1\sigma-1\pi_{1,2}$). The first and second stage ionisation potentials are much the same as for calcium.

But we recollect now that these lines $\lambda\lambda$ 4216, 4078 are two of the lines principally made use of by Adams in his determination of absolute magnitudes. Now the main difference between a giant and a dwarf of the same spectral type lies in their state of diffusion. The radius of the giant is much bigger than that of a dwarf of the same mass—say, 20 to 30 times for a star of the sun's mass of type K—and the value of gravity at the surface will be some 500 times smaller. This means that the pressure and density gradients in the outer parts of a dwarf star will be much more severe than for a giant star. Now the radiation leaving a giant star—whether in the continuous spectrum or the residual intensity in an absorption line—must have originated at much the same depth, reckoned in mass per unit area, as that leaving a dwarf. The radiation leaving a giant must therefore have originated in a region of much lower pressure than that leaving a dwarf; and that degree of opacity to the continuous spectrum which constitutes an absorption line will be established at a lower pressure in a giant than in a dwarf. We see to the same optical depth in the two stars; but owing to the smaller gradient in a giant the limit to our range of visibility occurs at a smaller pressure. It follows that there should be stronger ionisation in the effective layers of a giant, and therefore a stronger development of enhanced lines, in precise agreement with the observations of Adams.

We see that it is indeed the pressure which comes in as a second variable cutting across the spectral types. Adams' correlation of the intensity of enhanced lines with great intrinsic luminosity is an indirect one; the chain of correlation is strong ionisation—low pressure—low surface value of gravity—large radius—large disc area—large intrinsic luminosity.(25)

We can now account for the paradox that giants have lower effective temperatures than dwarfs of the same spectral type, and yet show a stronger development of enhanced lines. Temperature for temperature, the atmosphere of a giant is at a much lower pressure than that of a dwarf, and its spectrum will therefore be of a much earlier type. Consequently, re-classified on the basis of the general appearance of their spectra, the giants will have the lower temperature for the same general type. But the more conspicuous enhanced lines will still stand out. From this point of view it would be logically preferable for stars to be classified into temperature classes (colour classes); the changes of spectrum between giants and dwarfs of the same colour class would then be very striking; in addition, the differences would be more readily accessible to theoretical investigation.

Saha not only pointed out that the development of enhanced lines meant strong ionisation and that strong ionisation could be secured just as well by low pressure as by high temperature. Following Lindemann and Eggert, he applied the thermodynamic dissociation formula of physical chemistry to calculate the degree of ionisation of a gas at a high temperature in thermodynamic equilibrium. Self-ionisation at a high temperature is completely analogous to chemical dissociation, the work represented by the ionisation potential taking the place of the (negative) heat of reaction. The formula expresses the degree of ionisation as a function of the temperature and pressure.

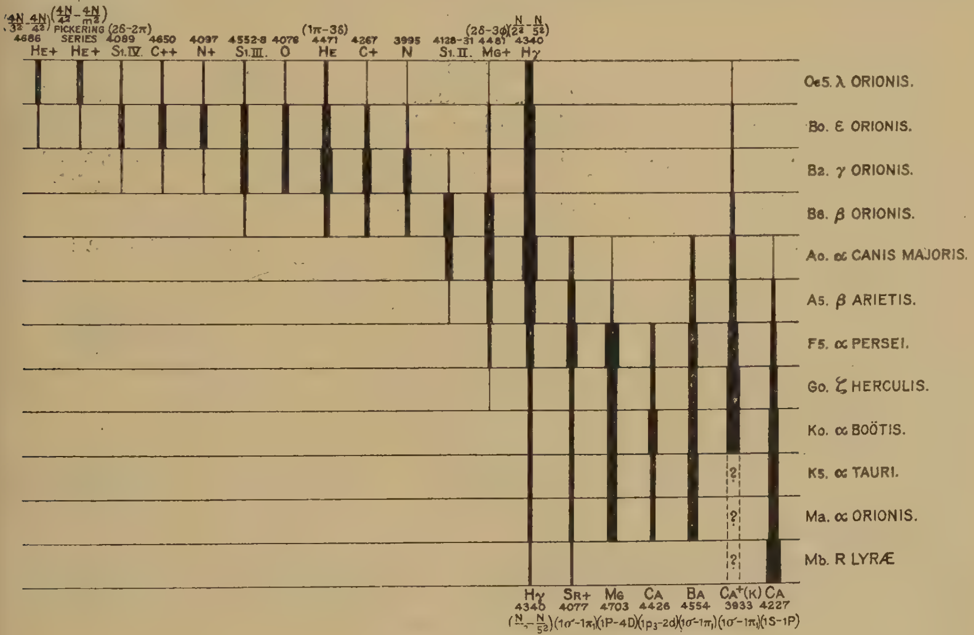
Adopting a value of the pressure of the order of an atmosphere to one-tenth of an atmosphere, and assuming that a line was on the point of appearance or disappearance when the proportion of atoms capable of absorbing it was some fraction like 1 or 0·1 per cent., Saha was able to calculate the temperatures of appearance and disappearance. For example, for the disappearance of $\lambda\, 4227$ Ca he found a temperature of 13,000°, and this should be the true temperature of the atmosphere of a star in which this line as an absorption line is just not visible. This temperature was therefore attributed by Saha to stars of type *B*8. Similarly he calculated the disappearance temperature of $\lambda\, 3933$ Ca^+ as 20,000°, of $\lambda\, 4215$ Sr^+ as 14000°.

I have presented the theory as it applies to the principal lines of an optical series; the principal lines of a neutral atom should steadily fade with increasing temperature, the principal lines of an ionised atom should rise to a maximum and then fade. The theory for lines of a subordinate series is slightly less simple. I have used the term principal series as synonymous with the series of those lines which are absorbed by the atom in its state of lowest energy. The Balmer lines, for instance, in this terminology are not principal lines; in order to absorb a Balmer line, a hydrogen atom must first be raised to the two-quantum state. At low temperatures the proportion of atoms in other than the normal state will be negligible. As the temperature increases, the proportion in any given excited state will increase. Thus, whilst the lines of a principal series can appear as absorption lines, however low the temperature, the lines of a subordinate series will only appear when the temperature is sufficiently high to give an appreciable fraction of excited atoms. Ultimately the subordinate lines must disappear along with the principal lines as the atoms become ionised. A principal line of a neutral atom has only a disappearance point, but a subordinate series line, like the principal lines and indeed all lines of an ionised atom, will have both an appearance point and a disappearance point, with a maximum in between.

As an example, the Mg sharp triplet $\lambda\lambda\, 5184,\ 5173,\ 5167$ $(1p_{1,2,3}-1S)$ increases in intensity up to Ko stars, thereafter decreasing. The Balmer lines steadily increase in intensity up to Ao stars, and then slowly decrease. The arc lines of helium, which are all subordinate series lines from this point of view, appear in stars of type Ao, reach a maximum in type B2, and disappear in Plaskett's type O5. The enhanced line $\lambda\, 4481$ Mg $(2\delta-3\varphi)$, which is a subordinate line of an ionised atom, first appears in the sun (type Go), reaches a maximum simultaneously with the Balmer lines, and disappears in type O7. The higher the ionisation potential, or the greater the energy level difference between the normal state and the appropriate excited state, the hotter will be the stars in which a given line is developed.

Broadly speaking, these considerations account for all the appearances, maxima and disappearances, the risings and wanings of intensity that occur in the stellar sequence of spectra, for lines whose series relations are known; and account for them further in their proper order. It is reasonable to suppose that the sequences of changes undergone by lines of the more complicated spectra, of carbon, nitrogen and oxygen, of iron and titanium, will be similarly accounted for in the future.

But from a quantitative point of view, there is so far something to be desired. It has been necessary to assume, somewhat arbitrarily, a value for the minimum concentration below which atoms in the suitable state do not yield an observable absorption line. And it has been necessary to assume, equally arbitrarily, a value for the pressure. Recently it has been found possible to avoid the first of these difficulties and to overcome the second, by concentrating attention on the position of maximum intensity of a line in the stellar sequence, instead of on the positions of appearance and disappearance.



$(\frac{4N}{3^2}-\frac{4N}{4^2})$ 4686 He+
$(\frac{4N}{4^2}-\frac{4N}{m^2})$ PICKERING SERIES He+
$(2\delta-2\pi)$ 4089 Si.IV.
4650 C++
4097 N+
4552-8 Si.III.
4076 O
$(1\pi-3\delta)$ 4471 He
4267 C+
3995 N
4128-31 Si.II.
$(2\delta-3\phi)$ 4481 Mg+
$(\frac{N}{2^2}-\frac{N}{5^2})$ 4340 Hγ
O e5. λ ORIONIS.
Bo. ε ORIONIS.
B2. γ ORIONIS.
B8. β ORIONIS.
Ao. α CANIS MAJORIS.
A5. β ARIETIS.
F5. α PERSEI.
Go. ζ HERCULIS.
Ko. α BOÖTIS.
K5. α TAURI.
Ma. α ORIONIS.
Mb. R LYRÆ
?
?
?
Hγ 4340 $(\frac{N}{2^2}-\frac{N}{5^2})$
Sr+ 4077 $(1\sigma-1\pi_1)$
Mg 4703 $(1P-4D)$
Ca 4426 $(1p_3-2d)$
Ba 4554 $(1\sigma-1\pi_1)$
Ca+(K) 3933 $(1\sigma-1\pi_1)$
Ca 4227 $(1S-1P)$

FIG. 1.

A diagrammatic representation—made by Mr. F. E. Baxandall from eye estimates—of the relative intensities of various absorption lines through the different types of stellar spectra. One of the chief objects was to portray the respective stellar types where each line attains its maximum intensity. The comparison of intensities is implied only in a vertical direction, i.e., the lines of varying thickness in any one horizon do not represent the relative intensities of the lines for that particular stellar type.

Consider, again, a line which is only absorbed by neutral atoms in an excited state, such as a Balmer line. For any given pressure, the proportion of neutral hydrogen atoms which are in the excited state will increase as the temperature increases. But the proportion of the whole atoms which are neutral will decrease as the temperature increases. The proportion of the total atoms which are in the excited state is the product of these two fractions. At low temperatures, almost all the atoms will be neutral, but hardly any will be in the excited state ; at high temperatures a large proportion of the neutral atoms will be in the excited state, but the number of neutral atoms will be small, and so, again, the number of excited atoms will be small. In between there will be a maximum. A formula of statistical mechanics, due originally to Boltzman, gives the fraction of neutral atoms which possess a given energy, and thus the fraction in any given state of excitation. Combining it with the ionisation formula, we can find the true proportion of atoms in the excited state at any temperature. By simple differentiation we can find for what temperature this has a maximum. For a subordinate line whose ionisation potential and series relations are known, the temperature of maximum concentration of atoms in a given state of excitation is found to be given by a definite formula in which the only unknown is the partial pressure of free electrons.(26)

We can plausibly identify the temperature of maximum concentration of excited atoms with the temperature of maximum intensity of the corresponding lines in the stellar sequence. The Balmer lines reach a maximum in stars of type Ao, with an effective temperature of about 10,000°. It is found that this will agree with the calculated temperature provided the pressure is taken as $1 \cdot 3 \times 10^{-4}$ atmosphere. This, therefore, should be the mean electron pressure in the layers of a stellar atmosphere which produce a strong hydrogen absorption line.

Assuming the same pressure holds in general, we can insert it in the formula and determine the temperature of maximum intensity for all other subordinate series lines. It is found that the sharp and diffuse lines of sodium should have a maximum at a temperature of 3,900°, diffuse triplets and sharp and diffuse singlets of calcium at 4,400°, the corresponding arc lines of magnesium at 5,300°, the arc lines of helium at 16,100°, and the Pickering series at 35,200°. By noting the observed places of maxima in the stellar sequence we obtain a scale of temperature ; or, reversing the procedure, we can assume the stellar scale known from other evidence, and calculate the pressures.

Roughly speaking, the resulting temperature scale is in agreement with that deduced in other ways. Perhaps the most striking confirmation of the theory is given by the enhanced line of magnesium, λ 4481 $(2\delta - 3\varphi)$ Mg^+. It is observed to have a maximum in stars of type Ao, simultaneously with the Balmer lines, and the calculated temperature is 10,200°. A definitely quantitative theory, utilising simply the optical term-values, predicts that lines so different in origin and series relationships as the Balmer lines due to the neutral hydrogen atom and the leading doublet of the fundamental series of ionised magnesium should have simultaneous maxima.

The calculated maximum of the arc lines of helium, 16,100°, fixes the temperature of type B2. Spectral theory thus confirms the higher of the various observational determinations. Up to the present the only apparent discrepancies occur with trebly ionised silicon and ionised helium. From Professor A. Fowler's investigation, the ionisation potential for the removal of the fourth electron from silicon is 45 volts. The lines $\lambda\lambda$ 4088, 4116, the leading pair of the second principal series, have a calculated maximum at 27,000°. They are observed to have a maximum in ϵ Orionis, of type Bo. The effective temperature of stars of type Bo is usually supposed to be much less than this. Again, the calculated temperature of 35,000° for the maximum of the Pickering series is perhaps rather high for the O-type stars of which these lines are a principal characteristic. It is probable, however, that the Harvard spectral divisions of the hotter stars correspond to quite large temperature steps; there is considerable progress, for example, inside the stars classified as Bo.* It should also be noted that the calculated temperatures would come out lower if lower pressures than 10^{-4} atmosphere are required for these lines of high excitation.

As further series relations become available, more and more points in the scale will be capable of determination, and we can look forward with great interest in this connection to the new results which we hope Professor Fowler has in store for us.

The assumption that the mean pressure at which an intense line originates is the same for all lines, at least as regards order of magnitude, is plausible, but so far it is pure assumption. If we take the usually accepted temperature scale from M to B, and determine the pressures so as to make the calculated temperatures of maxima agree, the most striking thing is the uniformity with which they cluster round 10^{-4} atmosphere; the variations are only what one would expect from the lines that are known to be high or low level lines in the sun. (The pressure given by the calculations is not the total pressure, but the partial pressure of electrons—it is this which governs the rate of capture of electrons by ionised atoms, and hence controls the degree of ionisation. But the total pressure will be in general of the same order of magnitude.)

The question arises, why are the pressures of this order? It used to be believed that the pressure in a stellar atmosphere amounted to an atmosphere or to several atmospheres. For faint lines, indeed, the pressure is likely to be higher than for strong lines, since in faint lines we see deeper into the stellar atmosphere. Saha's procedure, which depended on an assumed pressure of one atmosphere, gave results in accordance with observation, because he assumed an unnecessarily high concentration as the minimum for marginal appearance. The Balmer lines have their maximum when there is a concentration of one suitable atom in 10^5; Saha assumed a concentration of 1 in 10^2 or 1 in 10^3 for bare appearance. Thus results from marginal appearances are in no conflict in this respect. And all recent lines of investigation—from pressure shifts of wave-lengths(27) or from the disappearance, through dissociation, of a compound like TiO_2, which gives the flutings in M-type stars(28)—agree in determining small or practically zero pressures. But can we account on *a priori* grounds for the figure of 10^{-4} atmosphere?†

Even to begin to do this, much more refined considerations are required than those we have been adducing. The concentration of suitable atoms varies from level to level, and the resulting intensity of the spectral line depends on the superposed effects. A detailed theory of the structure of the atmosphere becomes necessary—we can no longer deal in mean pressures or mean concentrations. But it is clear that on any theory there is one physical constant that we shall require to know, namely, the absolute value of the absorption coefficient of the gas for the spectral line concerned. The bigger the coefficient, the more opaque the gas, and the less far we can see into the stellar atmosphere in the radiation of the frequency of the line; and so the lower the pressure. The absolute value of the pressure will depend on the absolute value of the absorption coefficient.

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* I owe this remark to Mr. F. E. Baxandall.

† Our value of 10^{-4} atmosphere refers only to lines of subordinate series. Atoms in the normal state in general heavily outnumber atoms in the higher quantum states, and hence lines belonging to principal series must be expected to originate at partial electron pressures much lower than 10^{-4} atmosphere. Some verification of this is afforded by the maxima of the principal series of ionised atoms (such as H and K for $Ca+$); for 10^{-4} atmosphere, the calculated temperatures of maxima are higher than the observed, and the two can be brought into agreement by taking a lower pressure, in accordance with the theoretical prediction.

Now, the absorption coefficient of the gas as a whole is the product of two factors: one is the concentration of atoms in the required state, the other is the atomic absorption coefficient for an atom in the required state. We have already taken account of the concentration. The next step is to take account of the absolute value of the atomic absorption coefficient. The order of magnitude of the pressure in a reversing layer must be fixed by the order of magnitude of the atomic absorption coefficient. Knowing this, we should be able to calculate the pressure; or, conversely, from the pressure we should be able to deduce the atomic absorption coefficient.

The atomic absorption coefficient depends on the probability with which an atom in a given state, exposed to given radiation, will absorb a quantum. Direct measures of monochromatic absorption coefficients are highly to be desired. The only observations are those of Wood, on the absorption by mercury vapour of the mercury resonance line. These indicate an absorption coefficient of the order of 10^9. Einstein has shown how the absorption coefficient is connected in a simple way with the average time τ during which an atom remains in an excited state, pending re-emission.(29) Clearly, the shorter is τ, the bigger is the probability of emission and the bigger is the coefficient of emission; hence, by Kirchoff's law, the bigger is the coefficient of absorption. It has been found by experiment that τ is of the order of 10^{-8} sec., and this value leads to optical absorption coefficients of the order indicated by Wood's experiments.(30)

The satisfactory determination of the absolute values of monochromatic absorption coefficients, or what is the same thing, the values of τ, from purely astrophysical evidence by way of pressures in reversing layers, has not yet been accomplished, but preliminary attempts indicate absorption coefficients of the order of at least 10^8 or 10^9, in agreement with Wood's experiments and with the value deduced from the experimental value of τ.(30) The validity of any calculations will have to be checked ultimately by their ability to predict the correct temperature differences between giants and dwarfs.

Eddington has shown that in the *interior* of a star such as Capella, the mean absorption coefficient (for all wave-lengths) is about 150. This means that a slab of stellar material of mass equivalent to 6 in. of air at atmospheric pressure would transmit only 1/20th of the radiation falling on one side.(31) He has commented, "the stars are thus amazingly opaque to radiation." But the monochromatic absorption coefficients concerned in the formation of absorption lines in a stellar atmosphere are ten million times as big. It is owing to these huge opacities that absorption lines appear to originate at such comparatively low pressures. Indeed, for such large absorption coefficients 10^{-4} atmosphere seems rather a large pressure, until we remember that atoms in the higher quantum states capable of absorbing a subordinate line form at most a very small proportion of the total number of atoms. The partial pressure of excited atoms must be very small indeed.

We have seen that low pressure goes with small values of g, the surface gravity. But low pressure goes also with small values of τ. It should be possible to disentangle these effects, since g depends on the star whilst τ depends on the atom.

Thus it is probable that whilst the general similarity of the pressure values deduced from the different lines is a reflection of the fact that τ is of the same order of magnitude for all lines of all atoms, the existing discrepancies may ultimately be traced to the differing values of τ for different atoms. If, for example, τ is much shorter for a highly ionised atom like trebly ionised silicon, we have an explanation of the anomaly already mentioned.

From the point of view of pure physics it is more interesting to note that we have potentially a method of evaluating from astrophysical evidence the absolute value of τ, a quantity only measurable in the laboratory with difficulty. I will conclude by mentioning an entirely different line of astrophysical evidence which is capable of furnishing τ.

I have already mentioned the chromosphere as a region of gas at a very low pressure, supported by radiation pressure. It was to the sun's chromosphere that Saha made his first application of the theory of the dependence of ionisation on pressure. The lines due to the ionised atoms extend to much greater heights above the reversing layer, in general, than lines due to the same atoms neutral. Thus Mitchell(32) found that the H and K lines of Ca^+ extended to 14,000 km., whilst the line λ 4227 of Ca extended only to 5,000 km. Similarly λ 4215 Sr^+ extended to 6,000 km., λ 4607 Sr only to 350 km. At the greater heights, the pressure is so low that gases like Ca and Sr with comparatively low ionisation potentials are completely ionised, and only their enhanced lines are visible. Hydrogen atoms, with their high ionisation potential, retain their electron even at great heights. On the other hand the enhanced line of Mg, λ 4481 ($2\delta-3\varphi$), is barely visible, whilst some of the arc lines of Mg extend to 6,000 km. This is, however, in accordance with theory, since though the Mg atoms will be largely ionised at the greater heights, comparatively few of them will have their electron in the 3φ orbit. The 3φ orbit of Mg^+ is 11·57 volts higher energy than the normal state of the Mg^+ atom, whilst the 1π orbit of Ca^+ is only 3·14 volts higher than the normal state of the Ca^+ atom; the non-appearance of λ 4481 at high levels is thus quite compatible with the strength of λ 3933.

Let us suppose that at high levels each ionised calcium atom is suspended in equilibrium under the balance of radiation pressure and gravity. Each time it absorbs a quantum of λ 3933 radiation it receives a certain impulse, and it is easily calculated that 10^4 of these impulses per second are required to keep it in equilibrium. If we knew the intensity of the radiation to which the atom is exposed and also the spectral range within which absorption is possible, we could compare the amount of energy absorbed in the 10^4 impulses per second with the amount available for absorption, and so calculate the absorption coefficient. But it is awkward to fix the "breadth" of the line and we can avoid the difficulty as follows :—

After each impulse the atom must return to the normal state to be ready for the next, and thus the average time of the completed cycle is about 10^{-4} seconds. If τ_1 is the average time each atom spends in the normal state, between emission and the next absorption, τ the average time it spends in the excited state, between absorption and the next emission, then the sum $\tau+\tau_1$ must be about 10^{-4} seconds. From thermodynamic considerations following Einstein, we can evaluate the ratio τ/τ_1 for any given intensity of radiation. (τ is independent of the intensity, τ_1 is inversely proportional to the intensity.) The breadth of the line is not required. Combining the ratio τ/τ_1 with the sum $\tau+\tau_1$, we obtain τ and τ_1 separately. The only uncertainty is the intensity to which the atoms in the high level chromosphere are exposed. But since an absorption line is formed, the intensity is at most less than in the neighbouring continuous spectrum.

The relations may be summarised in the form

$$\tau+\tau_1=10^{-4}$$

$$\frac{\tau}{\tau_1}=\frac{I_{\text{chrom.}}}{I_{\text{phot.}}}\times 6\times 10^{-4},$$

and since $I_{\text{chrom.}}<I_{\text{phot.}}$, we find at once that τ must be less than 6×10^{-8} seconds. If the intensity in the centre of the line is, say, 1/30th of the neighbouring continuous spectrum, then τ comes out as 2×10^{-9} seconds. An exact measure of the intensity in the line would furnish an exact value for τ, on our hypotheses; the chromospheric calcium must adjust itself, in fact, so as just to give the right intensity for equilibrium at high levels. The value derived for τ in this way for Ca^+ is of the order of the experimental values for hydrogen, and agrees with such other estimates as have been made, and it is somewhat remarkable that so much can be deduced from the simple qualitative observation that Ca^+ atoms appear to exist in equilibrium at high levels in the sun's chromosphere.

The application to other chromospheric lines, such as those of hydrogen and helium, which are not derived from the normal state of the atom, is more difficult and we omit its consideration here.

Thus astrophysical evidence is capable of estimating the atomic quantity τ for certain lines of certain atoms by two independent lines of argument; one based on the march of the intensity of the line through the spectral sequence of the stars, the other based on equilibrium in the chromosphere. Modern spectral theory has solved many of the problems of astrophysics; in return, astrophysics has once again something to contribute to the physics of the atom.

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DISCUSSION ON LOUD-SPEAKERS FOR WIRELESS AND OTHER PURPOSES.

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GENERAL PRINCIPLES INVOLVED IN THE ACCURATE REPRODUCTION OF SOUND BY MEANS OF A LOUD-SPEAKER.

By PROF. A. O. RANKINE, *D.Sc.*

THE task of opening this discussion is not an easy one. My difficulty arises, no doubt, largely from the mistake of supposing that a mere lively interest in the subject constitutes a sufficient qualification for making the introductory remarks. But I have also an uneasy feeling that there may be in relation to loud-speaking devices many important discoveries which continue to be withheld from public knowledge because of their commercial value. Consequently I may say things which might immediately be proved from practice to be wrong, if only everyone would reveal what he knows. The problem is, in fact, one in which practice has outstripped theory, as in the beginnings of aerial flight. A sudden great public demand has had to be met, and the problem has approached solution, so far as we know, by trial-and-error methods and without much appeal to the theorist. But few would deny that the products leave room for considerable improvement, except, perhaps, those responsible for advertisements (frequently to be seen) in which certain loud-speakers are described as " perfect in tone " and " absolutely faithful in reproduction." The real position seems to be that there is rather widespread discontent with the loud-speaking devices which are in use ; and the purpose of this discussion is, I take it, to endeavour to identify the defects and suggest means for their elimination.

I think that the most useful thing I can do—in fact, the only thing I am able to do—is to present certain considerations, mainly theoretical, which will be admitted to have an important bearing on the question before us. Some of them are rather obvious, and have probably already been taken into account by manufacturers. But few who have worked in acoustics have not, at some time or other, been faced with apparent contradictions between theory and practice ; it is wise, therefore, sometimes to pause and reconsider what is fundamental and true. First of all, what is the practical problem ? We wish to procure at one place the emission of sounds which are a sufficiently faithful copy of those originating at another place. It is not enough that the imitation should be agreeable ; we do not want, to take an extreme example, a piccolo played very badly to reappear as a performance on a bassoon completely above reproach. Whatever the original sounds are, we want the reproduction to be like them. Besides this—and it is here that apparently the chief difficulty presents itself—we require that the reproduced sounds should be of considerable intensity. Although there is no need to specify this intensity precisely, we may, perhaps, for the purpose of argument, say that the sounds emerging from a loud-speaking device (if it is to deserve the name) should be at least as loud as those emitted by the original source. In other words, the ideal loud-speaker would be a secondary source in every acoustical respect equivalent to the primary source, whether speaker, singer or musical instrument. Put into the language of mechanics this means that the vibratory movements of the air at any specified distance from the loud-speaker should be identical with those which would occur if the original source were substituted for the loud-speaker. This statement of the case, it is true, leaves out of account the important question of the special distribution of a large sound source such as an orchestra. It is not easy to see how any practicable loud-speaking device could imitate this effect. But this aspect of the subject has been deliberately ruled out of the discussion by its limitation to the field of reproduction, and I shall not break the rules in this respect. Nor shall I enter into psychological considerations further than to remark that it is conceivable—perhaps probable—that perfect fidelity of sound reproduction alone may never lead to complete satisfaction by compensating for missing factors. It is at least arguable that visual conditions are not entirely unimportant ; the sight of the lips or of the instrument manipulation may be needed to complete the effect.

The point at which our inquiry begins is, as I have already said, not earlier than the stage in which there are at our disposal, in electrical transmission at any rate, certain electrical fluctuations corresponding to the original sounds. The question is how to manipulate these electrical vibrations so as to procure a satisfactory copy of them in aerial vibrations of large amplitude. For I think that for present purposes we may take it that already we have available sufficiently good reproduction if we are content with feeble intensity. There is, in my view, little cause for complaint in the behaviour of good quality telephone receivers applied close to the ear and emitting sounds then just comfortably audible. Again, it will probably be generally admitted as true that the quality of the sounds given out even from a loud-speaker may be improved by reducing the output attempted. It is when large emission intensity is required that the distortion becomes too marked even for non-critical ears. I am no believer in the practical necessity of attaining the ideal case in which the aerial vibrations are copies, perfect in every detail of frequency, amplitude and phase, of those which gave rise to them. My own small experience has led me to the view that considerable latitude in this respect is permissible. On theoretical grounds, too, it would appear that to procure reproduction absolutely perfect in the physical sense, as distinct from the acoustical, is not possible, having regard to the variety of transformations which are in practice necessary. But there must be a limit for this physiological latitude, and there is evidently a danger, in seeking loudness, of departing too much from similarity. The various stages in the process each give rise to the possibility of distortion, and the effect is presumably cumulative ; thus each stage requires careful consideration.

Broadly speaking, there are, from the point at which the field of discussion opens, three operations. There is first the amplification of the electrical fluctuations ; in the second place there is the process whereby the current excites corresponding variations of air pressure ; and thirdly, we have the treatment of the aerial vibrations after they have been created. With regard to the first, I am not able to speak at any length ; it is a question of using one fluctuating current to impress its own features on another of greater power, usually in several successive stages. Most of us who have occasion to use thermionic amplifiers are aware that the later stages become more and more difficult, and we are fortunate to have the prospect of hearing later from Professor Fortescue what ought to be done. Perhaps he will be able to tell us also what are the limits of this type of amplification from the point of view of the subject in hand.

Our second question—that of the transformation of a portion of the electrica energy into sound energy—is so broad that it is difficult to choose a point of departure Each mode of transformation, according to whether it is by electromagnetic, electrostatic or thermal means—to mention only some of the better known—provides a different field for investigation ; and it would be impossible for me in the time at my disposal, even if I had the knowledge, to deal with them. In all cases, however, it is a question of forced vibrations, and I propose to make some general remarks on this subject which inevitably have a bearing on the problems of transmitting as well as those of receiving. I cannot do better than take as a text two quotations from Lamb's "Dynamical Theory of Sound":

> "The reason for the pre-eminent position which the simple harmonic type occupies in mechanics is that it is the only type which retains its character absolutely unchanged whenever it is transmitted from one system to another,"

and later, in elaboration of this point, after determining the forced oscillation under the action of a force composite only in the sense that it requires more than one different simple harmonic term for its representation,

> "This is an illustration of the remark made in par. 1 that the simple harmonic type is the only one which is unaltered in character when it is transmitted, the character of the *composite* vibration . . . being different from that of the generating force. In particular, if one of the imposed speeds p_1, p_2 . . . be nearly coincident with the natural speed n, the corresponding element in the forced vibration may greatly predominate over the rest."

Thus we cannot reproduce sounds in general with complete precision ; all we can do is to take steps to avoid too great changes of character in the often very complicated vibrations which we sometimes dissect for convenience into harmonic components. And the quotation just given directs attention to one of the chief dangers, namely, resonance. In aiming at loudness there is, no doubt, a temptation to resort to resonance as a means to that end. In the majority of telephone diaphragms, for example, there are natural frequencies where they ought not to be, i.e., within the range of frequencies of the sounds used, and the corresponding components of the sounds inevitably get preferential treatment. This, of course, can be rectified to some extent by damping the diaphragm, a process which diminishes the selectivity of the resonance by broadening the range of frequencies over which it occurs. But inevitably the general sensitivity is thereby reduced, and it seems to be at least worthy of greater consideration whether, especially in view of the fact that in operating loud-speakers it is usually possible to detect one or more diaphragm notes, advantages might not accrue from plans alternative to permitting resonance and only partially suppressing it.

There are two obvious ways of proceeding. One is to increase the natural frequencies of the mechanism to values above the upper limit of audibility, or at least as far in that direction as may be practicable. The other is to choose mechanisms of very low natural frequencies, so that none but the relatively high over-tones—which are not very liable to be excited—reach the lower limit of audibility. On theoretical grounds the former method is to be preferred, since it would give displacement components more closely corresponding to the exciting force. Where loudness is not important this procedure has, I believe, proved very satisfactory ; it remains to be seen whether the sensitivity necessary for loud-speakers is attainable under like conditions. Perhaps others with more knowledge of the mechanical properties of, for example, a diaphragm, will be able to say what are the prospects in this direction.

The other alternative, that of using mechanisms of very low, i.e., nearly zero, natural frequencies, is also worthy of continued consideration as possessing, at any rate, advantages over more resonant arrangements. One device of this type is to be exhibited at this meeting, and there is another, of which I heard not long ago, which presents certain features novel enough to be worth mentioning. It is attributed to Siemens Halske, and is said to consist of a strip of thin metal foil suspended between the poles of an electromagnet, as in the Einthoven galvanometer. The plane of the foil is parallel to the magnetic field, and the incoming telephonic current, doubtless properly transformed to suit the arrangement, flows through the foil. This responds by mechanical movements perpendicular to its plane, and is the equivalent of the ordinary telephone diaphragm. Its fundamental natural period is 2 seconds, or thereabouts, and it is reported to operate efficiently without a horn. I mention this instrument for the purpose also of directing attention to the fact that the diaphragm (i.e., the foil) suffers no transverse forces except those due to the telephonic currents. In this respect it differs rather fundamentally from the ordinary telephone receiver in which the diaphragm, or reed, is actuated by comparatively small increments and decrements of an attractive force of large magnitude, which is present in order that the sensitive part of the magnetisation curve may be utilised. Under these conditions a really sensitive diaphragm, i.e., one which at its centre responds with large displacement to the operation of a small force, seems to be ruled out, for it would be pulled over permanently into contact with the magnet poles. The modification of the system so that the diaphragm experiences no average force, as, for example, in the gramophone diaphragm, might lead to valuable results.

It is difficult to choose points for consideration from the many that present themselves. I will content myself with mentioning one more in this part of the argument, namely, the question of exciting forced vibrations in the air by means o an intermediate mechanism, a diaphragm being typical. It is said that in ordinary telephony only a very small fraction of the electrical energy received is convertec into sound energy. In the pursuit of intensity of reproduction there appear at firs sight to be grounds for high expectations in this direction, if only we could increas largely that proportion. I venture to suggest that it may not be wise to follow thi line too far, otherwise complications arising from reaction may appear. Our forcec vibration should presumably not be sufficiently energetic to be capable of alterin appreciably the character of the forcing vibration. This consideration applies botl to the current-diaphragm system and to the diaphragm-air system, if we have so t subdivide the process. I have sometimes wondered whether in this latter case departure from the above condition may be involved by using a horn in conjunctio with a diaphragm. With a horn there is an undoubted considerable increase in sounc output ; it is not merely a re-distribution of the sound in a particular direction Is it not possible that the horn increases too much the amount of energy transferrec by the diaphragm to the air, and thereby, quite apart from the well-recognised hor resonance, brings about distortion ?

I have only a few remarks to make with regard to the third subdivision, that is the treatment of the aerial vibrations after they have been developed. Of horn I will say no more than that they ought, if at all possible, to be dispensed with possibly for the reason just mentioned, more certainly because of their resonan character. Of course, they may have their uses in this respect if, after all, we hav to rely on resonance for intensity, and to adopt the plan of multiplying resonance so as to cover the range of frequencies used in transmission, thus making the best of a bad job. But the ideal sound radiator would be a spherical one which in some way could be excited by the electrical vibrations so as to impart to the neighbouring air symmetrical fluctuations of pressure of large enough amplitude. I am sorry to say that I have no practical suggestions to make on this basis.

A problem of importance is that of the conditions of listening to a loud-speaker, as, indeed, it is also in listening to a live speaker. It seems reasonable to suppose that in either case ideal listening would consist of hearing speech, at any rate, by the direct effect alone, without any reverberation or echo. For this we should have to damp out all room reflections both at the sending and the receiving stations. This is, in effect, frequently done in broadcasting, if we limit the case to that of listening on receivers applied close to the ear. But a considerable proportion of listeners appear to be asking for echo effects. Although I do not agree with them I can admit the argument, more particularly in relation to music as opposed to speech, that the custom of listening in a hall has led some to prefer the admixture of a certain amount of reverberation. No doubt Mr. Sutherland will deal with this and similar questions. All I would point out now is that in practical circumstances, if we admit the desirability of appropriate "room effect," as it is sometimes called, we must recognise that the problem is different according to the mode of listening, whether with head-receivers or with a loud-speaker. With the former the echo effect, if wanted, must be imparted at the transmitting station; with the latter it is liable to develop in the listening room also. Doubting as I do the value of even one system of echoes, I cannot be expected to tolerate two different ones superimposed, such as would arise, in the absence of suitable precautions, in a room or hall in which was operating a loud-speaker emitting sounds already bearing an echo impress. My submission is that for loud-speakers, as a general rule, echoes and reverberation should be eliminated at one end at least. For broadcast opera, for example, where transmission already unavoidably has the effect, the listening room should be draped much in the same way as transmitting rooms usually are.

I should like to say a few words in conclusion with regard to methods of testing results. Audition as a test of the degree of perfection of reproduction is liable to be a matter of opinion as between one person to another. From a scientific standpoint it would be much better to adopt a plan of taking simultaneous records of a visible type both of the original sounds and their reproduced copies, and comparing them. Even if feasible, however, I am not convinced of the value of this method except as a matter of scientific interest. Unless we could attain the ideal of identical records, a degree of attainment probably quite unnecessary for present purposes, and, indeed, theoretically impossible, we could be sure of nothing. After all, the problem is to deceive our auditory mechanisms by offering imitations; a listening comparison therefore constitutes the most direct test. It ought, of course, to be as nearly as possible a direct comparison; the original and the reproduced sounds ought to be capable of being heard not, perhaps, simultaneously, but at least alternately at frequent intervals. Our problem will be solved when the inventor is able to deceive the most critical ear, so that its owner does not know whether he is listening to the original or to the copy. It remains to be settled who is the severest critic. Those of us who have heard his recent lectures, and know of his almost uncanny faculty of mentally performing Fourier analyses, would probably nominate Sir Richard Paget.

THEORY OF LOUD-SPEAKER DESIGN: SOME FACTORS AFFECTING FAITHFUL AND EFFICIENT REPRODUCTION.

By L. C. Pocock, *B.Sc.*, *A.M.I.E.E.*

IF it is assumed that properly amplified and undistorted speech voltage is available in the output circuit of a final amplifier, the problem is to procure the reproduction of speech efficiently and faithfully. The exact criteria for the reproduction of speech are better known than for music, but it is probably safe to say that a system capable of reproducing speech perfectly will give a highly satisfactory performance with music.

If V is an impressed voltage of any frequency or amplitude within the region to be amplified without distortion, and P the resulting alternating air pressure outside the system, the conditions are

$$P=AV$$

where A is an efficiency constant independent of the frequency and amplitude. It is also necessary that there shall be no asymmetric distortion, that is, any single frequency V must produce only the corresponding single frequency P. This condition is also expressed by the equation above.

Present-day electromagnetic loud-speakers are, without exception, a compromise between relatively good efficiency and good quality, such efficiency as can be secured being obtained only with the aid of mechanical resonance, which is contrary to the criterion for faithful reproduction given above. Further, although telephonic speech has generally been handled in the past as a steady-state problem, recent improvements in transmission have rendered the transient phenomena associated with consonant sounds and every change of amplitude of some importance. The reproduction of severe transients cannot be perfect in any resonant system or in any system containing mass and stiffness, even though the damping be such as to prevent any natural oscillation; the severity of transients actually encountered in speech is dependent on the damping of the vocal resonances, and information on this subject, together with like information on the auditory mechanism, might indicate the desirable degree of damping from the point of view of transient phenomena. It is clear that the use of resonance to increase the efficiency cannot be pushed too far.

Practical loud-speakers consist of a rather sharply resonant system working into an acoustical load, namely, a horn. It is not quite accurate to describe the horn as a load, because the useful work is the energy transmitted through the horn. The horn is operating in a capacity analogous both to an electrical transformer and to an electrical transmission line. The likeness to a transformer is seen in the passage of energy from the high mechanical impedance of the diaphragm to the low impedance of the open end through a coupling device, which reduces energy reflection to a minimum and aims at obtaining the greatest possible transfer of energy. The likeness to a transmission line lies in the propagation of waves across the non-uniform section of the horn; the analogy is to a non-dissipative line containing distributed inductance and capacitance, the line constants changing steadily from end to end of the line in such a way that the impedance measured at one end of the line is high, and that measured at the other end is low. Such a system would form a maximum energy coupling between two different electrical impedances.

The acoustical impedance of a horn at its small end depends a good deal on the cross-section and also varies with the solid angle and the form of the horn, but, as in the electrical analogies, the impedance is also a function of the impedance into which energy is delivered, i.e., at the open end. Another view of the acoustical impedance at the small end is to regard it as the impedance of the large end modified by the horn through which it is measured. In general, the horn impedance also varies with frequency, and, though horns of approximately uniform impedance can be made, it is clear, from a consideration of the varying mechanical impedance of the diaphragm, that such a horn is not necessarily the best.

These are some of the factors which enter into the performance of a horn. The practical considerations are usually those of size; for indoor use the horn must not be too long, so that the problem is equivalent to attempting to obtain an electrical line of length equal to a wave-length or less and having a very much higher line impedance measured from one end than when measured from the other end. The acoustical impedance is virtually coupled to the diaphragm, so that some idea of its variations with frequency can be obtained by observing the motional impedance of the receiver. A large number of horns, of a size suitable for use in private houses have been examined in this way, and resonances of varying degree have been found in all; larger horns might, however, be expected to show lesser effects.

The resonance of a receiver without horn may be such that the diaphragm vibrates with more than 50 per cent. of the amplitude at resonance over a frequency region about 100 periods wide. When the horn is put in place, the diaphragm is made to do more work and the resonance is made much less sharp. The new damping coefficient cannot be simply expressed, because the resonance is no longer simple, but is complicated by the coupled horn resonances.

The actual pressure variation in the air when the receiver is excited at different frequencies can be measured. Figs. 1 and 2 show the characteristics of two types of receiver. The curve in Fig. 1 is for a flexible diaphragm driven by a small armature supported on a spring. The effective moving mass is not appreciably greater than that of the ordinary telephone receiver. The curve is an average of the results of five receivers and shows definite peaks in the lower frequency region, due to the horn. It is seen that the distortion due to these resonances is small compared with the general effect, due to the mechanical resonance of the system. This is an important point: Horn distortion can be brought within reasonable limits; the receiver mechanism is often responsible for defects of tone for which the horn is blamed.

In connection with Fig. 1 it may also be stated that the perfection of reproduction is a great deal better than the appearance of the characteristic would suggest; the contracted logarithmic scale disguises the really rather gradual fall of the curve at the higher frequencies; even at the extreme end of the curve the highest frequency shown is reproduced with sufficient intensity to add greatly to the quality of reproduction.

Fig. 2 is the characteristic of a loud-speaker of the iron-diaphragm kind, similar in principle to the ordinary telephone receiver; in this case the curve is an average of several tests taken on the same receiver. The frequency of maximum response

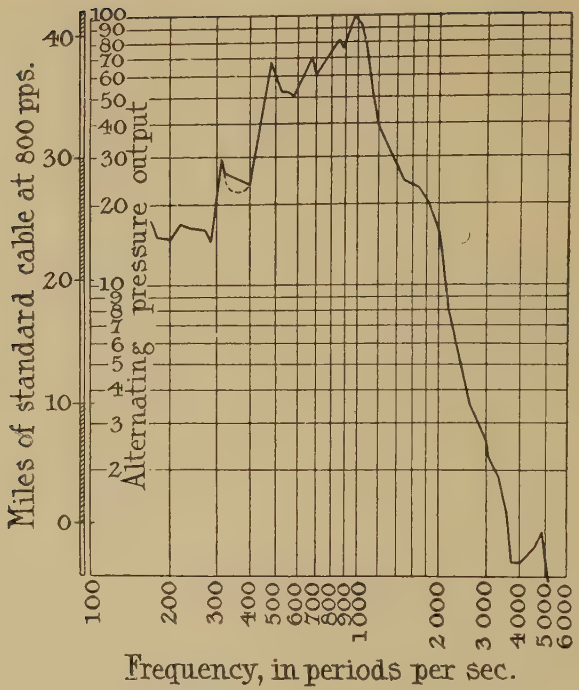

Fig. 1.—Alternating Pressure Output of Loud-Speaking Receiver corrected for Impedance of Circuit and Receiver. (Average of 5 receivers.)

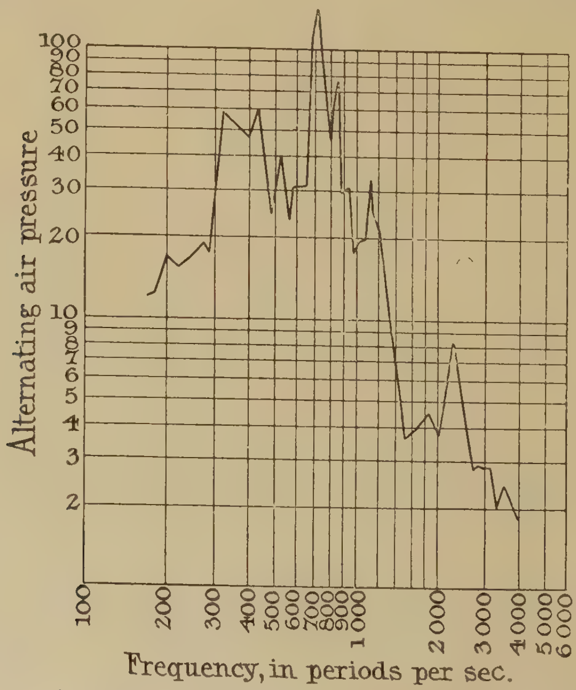

Fig. 2.—Alternating Air-Pressure Variation with Frequency for an Iron Diaphragm Receiver, corrected for Impedance of Circuit and Receiver.

is seen to be a little lower than in Fig. 1, and the curve drops somewhat steeply between 1,000 and 2,000 periods per second (p.p.s.).

In both the above cases the receiver output is corrected for the impedance of the associated amplifier, that is, a fixed voltage is operating on the loud-speaker through a fixed resistance representative of the amplifier output impedance that would be suitable for use with the receiver considered. Since the impedance of most receivers at about 4,000 p.p.s. is two, three or more times as great as the impedance at 1,000 p.p.s., the reproduction of the higher frequencies is somewhat impaired due to this cause.

It must be recorded that Figs. 1 and 2 are relative in value, rather than absolute ; in particular no correction has been applied for the variation with frequency of the absorbing power of the material used in the highly-damped chamber in which the measurements were made.

Receivers have been constructed in which large vibrating surfaces are used without a horn. It appears that the vibrating surface must be of such dimensions that there is difficulty in securing the necessary lightness of the moving parts, especially when the added mass, due to the reaction of the air, is taken into consideration. In any case, the very important distortion due to the use of mechanical resonance to obtain good efficiency remains in evidence.

With regard to the mechanical construction of an electromagnetic receiver, the ordinary construction of a telephone receiver requires considerable modification if it is to handle more than a very small amount of power and, even when so modified, there is danger of distortion due to the asymmetrical forces called into play by the passage of symmetrical currents. A receiver of the type giving the characteristic shown in Fig. 1 is capable of handling about 10 watts without asymmetrical distortion, because the armature is driven by symmetrical forces. The amplitude of vibration may be of the order of 0·01 inch.

To sum up, with present-day constructions of receivers, faithfulness in reproduction cannot be obtained beyond a certain degree without making receivers very inefficient. Reproduction can, by careful design, be made very satisfactory, but to obtain the very last degrees of perfection—e.g., by filters—enormous increases in the power amplification would be necessary to operate the receiver, in fact, valves of far higher power capacity than are used in any radio receiving sets. As it is an easy matter to obtain the present amount of amplification, it is seen that the chief interest in raising the efficiency of loud-speakers is to permit the application of quality-correcting devices, provided of course that increased efficiency is obtained without sacrifice of quality.

With regard to the overall efficiency obtained in loud-speaking receivers, it is probable that 1 per cent. is a high estimate and that a few tenths of 1 per cent. would generally be nearer the mark. The principal loss is iron loss, and though eddy currents may be reduced by lamination, hysteresis still accounts for a very considerable loss on account of the high frequencies concerned. It does not seem likely that any great improvement in real efficiency can be obtained unless a magnetic material with exceptionally low hysteresis loss and good permeability is discovered. Small improvements are possible by building receivers on a larger scale and using more powerful magnets, but the necessity of making some part of the moving system of iron and of low mass makes the employment of high alternating flux density in this vital part unavoidable.

THE SOURCES OF DISTORTION IN THE AMPLIFIER.

By Prof. C. L. Fortescue, *M.A.*, *M.I.E.E.*

I. Scope.

In this note the output P.D. from the rectifying valve or crystal is taken as the starting-point. With an ideal amplifier this P.D. is magnified and a current of precisely the same wave-form as the output P.D. from the rectifier is supplied to the loud-speaker. In many actual amplifiers, however, the wave-form is not faithfully reproduced and distortion is introduced.

II. The Causes of Inaccurate Reproduction.

These may be put under the following headings :—

(*a*) Curvature of the valve characteristics.
(*b*) The use of intermediate circuits having more or less clearly defined natural frequencies.
(*c*) The unavoidable reaction effects present in most designs of note magnifiers.
(*d*) Unsatisfactory reproduction in the last (or output) transformer.

III. The Effects of Curvature of the Anode Current Characteristics.

(*a*) *Resistance Amplifier.*

The ideal resistance amplifier is as shown in Fig. 1, and consists of a valve with a non-inductive and capacityless resistance, R_A, in series with the anode and a condenser of very large capacity across the battery terminals. The valve characteristics may be conveniently plotted as a characteristic surface in terms of V_B and V_G, allowance being made for the resistance R_A.

The surface shown in Fig. 3 is the ordinary characteristic surface, the lines corresponding to constant anode current, but allowance is made for a series resistance of 10,000 ohms. The fluctuations of the grid P.D. above and below the mean value may be plotted below the diagram of Fig. 3 as at *G*. Then, by projecting up to the line *PQ*, corresponding to the given value of the battery voltage, the values of the anode current can be plotted above and below the mean value at *C*. A reference to Fig. 3 shows that the anode current wave-form can only be an exact replica of that of the grid P.D. when the constant-current lines are equally spaced along the line *PQ*. Thus, if the surfaces are plotted out for any given valve, the possible range of anode current and grid voltage over which faithful reproduction can be obtained will be easily seen and the appropriate values of V_{G0} and V_B can be chosen. The values taken in plotting Fig. 3 are $V_B=200$, $V_{G0}=-4$. The amplitude of the fluctuations of V_G is 3·5 volts and of i_a, 1·75 mA.

(*b*) *Transformer Amplifier.*

Except in the last stage, a transformer in the anode circuit should closely approximate to a resistance. When very heavily damped, due to its own losses and the load of the valve, and when near the resonant point, this is actually the case. The effective resistance to the alternating P.D.'s (which are the ones under consideration) is, however, very much greater than the resistance of the anode winding as measured by direct current. The resistance must be ascertained by alternate-current bridge methods at the resonant frequency. Some transformers having a direct-current resistance of the order of 2,000 ohms are found at the resonance point to have effective resistances of 200,000 to 300,000 ohms when loaded on the secondary side with resistances corresponding to the grid resistance of the next valve. The representative characteristic surfaces must therefore be plotted for this high value of R_A and not for the direct-current resistance; the latter is only used to obtain the effective starting-point for any actual battery voltage.

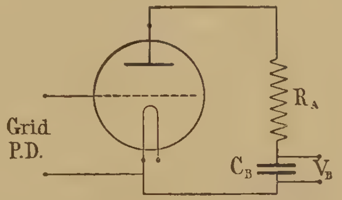

FIG. 1.—RESISTANCE AMPLIFIER.

For frequencies other than the resonant frequency of the transformer the conditions are more complicated, and merely plotting the characteristic surface with a correction for a series resistance is insufficient. The surface must be plotted without correction and both the grid and anode fluctuations must be allowed for. The line PQ of Fig. 3 becomes a curve—an ellipse in the case of two pure sine waves—and so long as this curve remains within the zone where the constant-current contours are equally spaced the reproduction will be satisfactory.

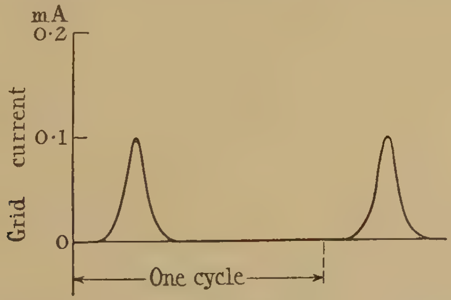

FIG. 2.—WAVE-FORM OF GRID CURRENT WITH CONDITIONS OF FIG. 3.

IV. EFFECT OF CURVATURE OF THE GRID CHARACTERISTICS.

If the grid voltage fluctuations have any considerable positive values the grid currents will be quite appreciable, and the wave-form of the grid current will differ very widely from that of the grid P.D. Fig. 2 shows approximately the curve of grid current corresponding to the conditions assumed for Fig. 3. The grid currents will generally react on the source of P.D. and lead to heavy distortion. With many amplifiers the peak of the positive half-cycle of the grid-filament voltage wave is practically cut off at $V_G=0$.

The only way of avoiding this difficulty is to render the effect of the grid current negligible. Valves have not yet been produced in which the grid current is negligible when the grid is positive and the anode voltage low, and consequently positive values of the grid voltage must be avoided. This gives another limitation to the range of the anode current characteristic curves that can be used, and indicates that the anode battery voltages should be high, and that the mean grid voltages should be considerably negative.

V. EFFECT OF THE NATURAL PERIOD OF THE INTERMEDIATE CIRCUITS.

This trouble arises in the case of a transformer amplifier. In the first place, any marked resonance means that the effective impedance in the anode circuit is dependent upon the frequency. The impedance—and therefore the amplification—will be greatest at the resonant frequency. Thus any sustained harmonic having this frequency will be unduly pronounced and the speech will appear "tinny" or "drummy," depending upon the pitch of the accentuated harmonic. The larger the number of stages of amplification that are used the more marked is the effect.

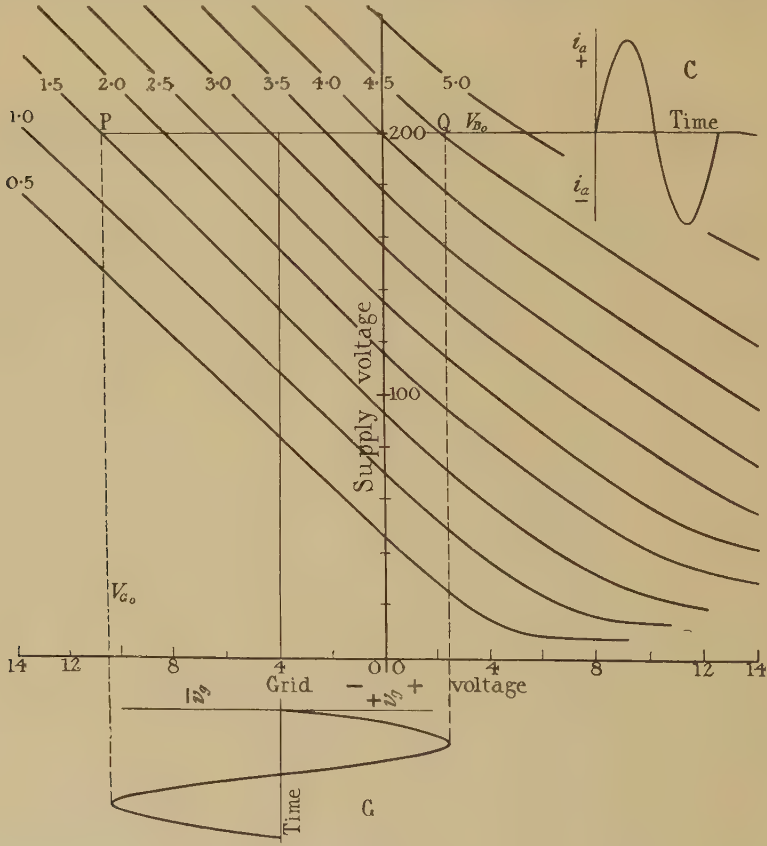

FIG. 3.

In the case of those high-frequency components which are not sustained, the effects are less pronounced. This effect is thus most noticeable with musical sounds and with the vowel sounds. Secondly, for frequencies other than the resonant frequency, the transformer is no longer equivalent to a resistance, and complications arise from the relative phase of the grid and anode potential fluctuations on account of which it becomes very difficult to determine the wave-form of the anode current when the amplitudes are fairly large.

VI. REACTION EFFECTS.

It is well known that reaction plays an important part in all amplifiers. In the note magnifier the effect is to accentuate the resonance effects mentioned in the previous paragraphs.

VII. DISTORTION IN THE LAST STAGE.

The last stage is not unfrequently a source of serious trouble for two reasons :—

(i) The amplitudes are large.

(ii) The "load" on the output transformer—viz., the winding of the loud-speaker—is inductive and this inductance is not constant.

With regard to (i), the output required for a sustained musical note is of the order of 10 mA (R.M.S.) at 5 volts (R.M.S.). To give an equivalent volume of sound with ordinary speech a peak value of perhaps double these figures will be necessary, and after allowing for the losses in the transformer it seems that the output from the anode circuit of the last valve will be equivalent to an alternating current of peak value 30 mA at an alternating P.D. of peak value 15 volts. A transformer is almost invariably used and the actual values would more probably be 10 mA at 45 volts. This involves a valve giving an emission current of perhaps 50 mA with a fluctuation of anode current over the range 15 to 35 mA ; and a voltage at the anode of perhaps 120, fluctuating between the limits of 75 and 165. General numerical considerations such as these show the necessity for valves of considerable output in the last stage.

High battery voltages are also necessary—in the above case the steady fall of P.D. in the anode circuit would be of the order of 50 to 100 volts, and a battery giving something in the neighbourhood of 200 volts would be unavoidable.

With regard to (ii), owing to the inductive nature of the load the last stage cannot be regarded as being even approximately a resistance, and the same effects are noticed as with a transformer operating out of resonance.

VIII. CONCLUSION.

With properly designed valves and circuits it does not appear that any serious distortion can be charged against the amplifier. Valves giving considerable power output must, however, be used in the last stage.

This conclusion has been verified by an oscillographic investigation of a two-stage transformer amplifier. A cathode-ray oscillograph was used, the circuits being given in Fig. 4. In the first instance the output voltages from the oscillators were applied to the deflecting plates of the oscillograph and gave a good ellipse on the screen, thus showing that both were good sine waves. This ellipse is shown in Fig. 5. The amplifier was then brought into use and the output wave when properly adjusted again gave an ellipse as shown in Fig. 6, proving that the sine wave-form was preserved. Unsuitable adjustments or excessive amplitudes showed very marked departures. In Fig. 7 the amplitude was too large and the sharp cut-off in one direction is due to the grid current effectually preventing the grid from becoming appreciably positive. Fig. 8 shows the ellipse closed in at one end owing to bad adjustment giving a mean position near the bottom of the curves of anode current.

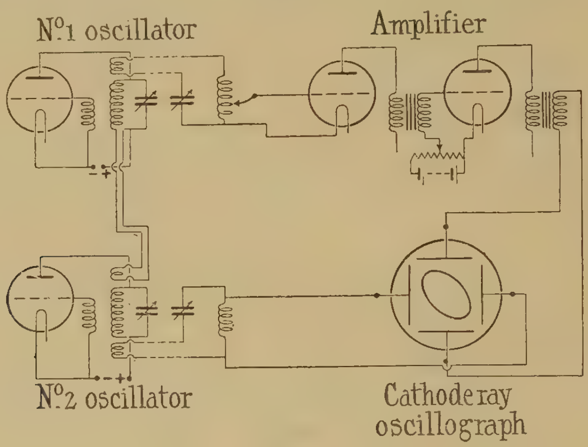

FIG. 4.

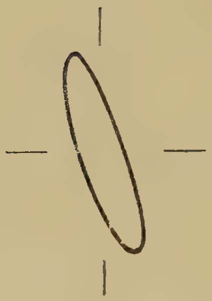

FIG. 5.

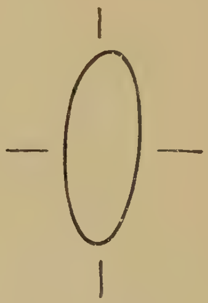

FIG. 6.

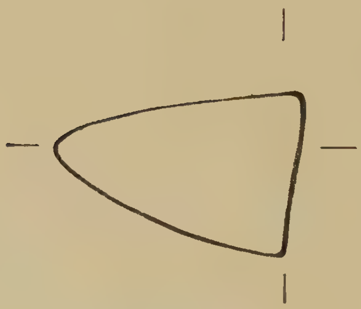

FIG. 7.

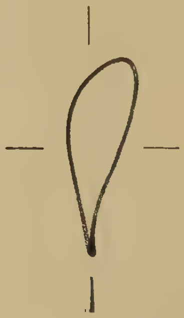

FIG. 8.

THE ACOUSTIC PROBLEMS OF THE GRAMOPHONE.

By H. L. PORTER, *B.Sc.*

IN this brief Paper it is proposed to give an outline of the work done on gramophone reproduction by P. Rothwell and myself, whilst in the service of the Gramophone Company, Hayes, Middlesex. It is hoped that in this way the problems and difficulties of the subject will be made clear. I am limited by certain restrictions in that the details of particular methods used cannot be published, neither can the lantern slides exhibited be reproduced. The general method and results which can be given may, however, prove useful in the consideration of the analogous problem of the loud-speaker The gramophone problem is acoustical and mechanical in nature ; in the latter respect it differs from that of the loud-speaker. For the purposes of this discussion I will confine myself almost entirely to the acoustical problem.

The simplest form of commercial recording apparatus consists essentially of a single conical horn in conjunction with a diaphragm. If such an apparatus be set up and its response to a series of simple tones emitted by organ pipes be measured by a suitably mounted mirror attached to the centre of the diaphragm, the characteristic curve shown in Fig. 1* is obtained, the amplitude of vibration being plotted against frequency of pipe. Preliminary work had been done on the simplification of room acoustics and maintenance of uniformity and purity in the tones. Curve 1 may therefore be accepted as a typical curve under these conditions. The response is very irregular, and it is clear that such an apparatus as we have used would give a very imperfect record. In commercial practice the matter is much worse, for the following reasons :—

(1) The acoustics of the recording room is an unknown and complicated factor.
(2) Many tones are recorded simultaneously.
(3) The tones are complex and are very rarely uniform in intensity.
(4) The characteristic curve is further modified by recording on wax.
(5) In reproduction no needle could possibly pass along the large-amplitude waves in the high peaks, so that further modifications have to be made with further loss.
(6) The reproducing apparatus has its own characteristic response.

The final curve of reproduction from a commercial record must therefore be much worse than shown in Curve 1 and in view of this it is surprising that gramophone reproduction is as good as it is. Doubtless the ear is very generous in its behaviour in this respect.

From a study of this curve and many similar ones we decided that the gramophone problem was not, as it had been in the past, a question of improving the "sound box" (as the diaphragm and its housing is termed) independently of the recording horns, or of improving the recording horns independently of the sound box, or of both independently of the room conditions. The combination—and here the room must be included—had to be considered as one acoustic system and treated as such. We therefore took a system in its simplest form and sought to arrange its response so that it should be uniform in terms of sound energy over the great range for which the gramophone must cater. This meant approaching the problem in three stages: First, to find to what each large response or peak was due; secondly, to ascertain whether we could control these responses, and, if so, to arrange them to give as much uniformity of response as possible; and thirdly to reduce the peaks to a suitable value and, if possible, raise the response in the troughs and so smooth out the curve.

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* The figures referred to in this Paper were shown as lantern slides.

Considering these in turn, the "placing" of the peaks led us to an examination of the influence of the diaphragm, its housing, the needle system, the connecting tubes, the recording horn and the room. Each played its part. It was not an entirely easy matter, for the slightest modification in the system often brought about big local changes due to the acoustic proximity of some other factor. However, each peak was successfully "placed," and we found we had gained during the course of the work a good deal of control over the whole system. Next we tried to arrange these peaks in some sort of order, and Fig. 2 shows the result we obtained.

There is a very important point to bear in mind here. If any system is set up in a haphazard manner, i.e., without any acoustical knowledge of the system whatever, one is more likely than not to get two peaks (due, of course, to different factors) falling together or near each other. When this happens a curious placing of peaks occurs and a tremendous massing of response, out of all proportion to the rest of the response, takes place over quite a number of neighbouring tones. In addition it often results in a large reduction of response in regions near at hand. This is shown clearly in Fig. 3.

It was also found that the recording apparatus would react upon the source of sound itself and modify its pitch. Again, it was found difficult to keep the recording apparatus constant in its response for some of our work. The deviations were not such as to affect the general form of the curve, but for later work any changes at all were very serious. Thus temperature had its influence, and many idiosyncrasies of the apparatus had to be investigated before we were satisfied that readings could be repeated from day to day.

Finally we had to eliminate the peaks in the general response curve obtained so far. Mr. Rothwell evolved a very useful plan to reduce these peaks and fortunately, in so doing, found that by the same method he could raise the response in some of the troughs. The final curve for the simple tones is shown in Fig. 4.

The readings from which this curve was drawn are, as in all the curves, amplitudes as ordinates against frequencies as abscissæ. The amplitudes are read on the scale from the deflection of the mirror. Much work was, however, done in tracing out the amplitudes on the waxes, matrices and final records. After certain minor corrections were applied to the system, Fig. 4 can be taken to represent the actual amplitudes obtained on the record. So far, then, we felt satisfied that our corrected system gave us a very good approximation to uniform response over a large range of frequencies. We were not sure, however, whether the balance of response given by Fig. 4 was true for normal hearing; that is to say, we may have allowed for too much in the lower frequencies and too little in the higher, or vice versa. Only a practical trial would settle that. We, therefore, proceeded to record. We chose the pianoforte as it presented many elements of difficulty and had a great range of frequency, and, moreover, we had made an attachment so that the instrument could be struck one note after another with an even, definite, blow which could be repeated. Here we were confronted with two serious difficulties, one of which we had not anticipated. We expected, of course, that the presence of such a large obstacle as a pianoforte in front of our recording apparatus would make a great difference and we found this to be so. We were troubled, however, to find that the impulsive nature of the pianoforte note was so different in its action from the steady uniform note of the organ pipes, since it made the elimination of the response peaks much more difficult. At last we made a record of the whole series of pianoforte notes where the amplitudes on the records were theoretically of uniform intensity ; only a few notes at the top were missing. When we played this record on a gramophone we were confronted with the other half of the problem. We found that the gramophone itself had its own peculiar response, which greatly disturbed the carefully graduated tones on the record. The loud responses and the general distortion depended upon the reproducing instrument used, some instruments being much worse than others.

THE RELATIVE IMPORTANCE OF EACH FREQUENCY REGION IN THE AUDIBLE SPECTRUM—MEASUREMENTS ON LOUD-SPEAKERS.

By E. K. Sandeman, *B.Sc.*

The Relative Importance of Each Frequency Region in the Audible Spectrum.

THE system of ideas bearing on the question of the criteria involved in sound reproduction, which is outlined below, is by no means to be regarded as absolute, but is merely an attempt to classify the types of sound characteristic to be preserved and the relative importance of the corresponding physical properties of sound.

The relative importance of each frequency in the audible spectrum depends on what function of hearing is to be served.

There are three essential characteristics of reproduction which naturally occur to anyone considering the matter :—

1. (1) The first is probably intelligibility of speech ; it must be possible to exchange ideas with reasonable facility.
2. (2) The second is naturalness of reproduction of music (and also of speech), which, as we shall see, is a more searching requirement.
3. (3) The third is the reproduction of speech at correct volume, which is bound up with the first two requirements and involves consideration of the energy of speech.

It is perhaps a surprising result in the case of speech that the importance of a frequency region, from the point of view of intelligibility, is by no means proportional to its relative importance from energy considerations.

The first requirement of intelligibility is that each isolated syllable shall be correctly interpreted. Syllable articulation may be defined as the percentage of such syllables correctly understood when read from a series of lists of unconnected syllables.

Similarly, word articulation may be defined as the percentage of words correctly understood, while intelligibility is the percentage of ideas correctly interpreted.

Fig. 1 shows the effect upon articulation and the energy of speech of eliminating certain frequency regions.

The full-line curve sloping up to the right shows the effect on articulation of transmitting only those frequencies below the point plotted, and so is of the form

$$A=\int_0^f F(n)dn \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (1)$$

where $F(n)\times dn$ is the percentage articulation carried by the frequency region n to $(n+dn)$.

The full-line curve sloping down to the right shows the effect on articulation of transmitting only those frequencies above the point plotted, and so is of the form

$$A=\int_f^\infty F(n)dn \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (2)$$

Differentiating, we get

$$\frac{dA}{dn}=F(n) \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (3)$$

so that the slope of the curve is a measure of the importance of each part of the spectrum in carrying articulation.

If we try to interpret Equation (3) rigidly we are brought face to face with the fact that the slope of the articulation curve for the passing of high frequencies is different from that for the passing of low frequencies. This seems at first to be inexplicable or else due to an error in observation on the part of those who made the tests. The following considerations will, however, make this clear.

By the above treatment we have tacitly assumed that articulation may be expressed as a function of frequency such that the articulation carried by any two regions is the sum of the articulations carried by each individual region. This is actually not the case, since articulation depends on the relative efficiency of reproduction of each frequency region. Hence a discrepancy creeps in if this fact is not allowed for, and practically it is not convenient to do this; actually it will be appreciated from these curves that the error introduced is not serious.

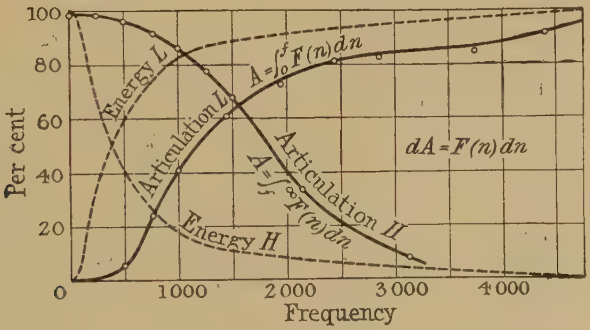

FIG. 1.—EFFECT UPON THE SYLLABLE ARTICULATION AND THE ENERGY OF SPEECH OF ELIMINATING CERTAIN FREQUENCY REGIONS.

The dotted lines show the energy function in the same way as the articulation. It at once becomes evident that the greatest slope of the articulation curves is not at the same frequency as that of the energy curves; in other words, those frequencies that carry greatest energy are not proportionally important from an articulation point of view.

There is as yet no information on the function of frequency portraying naturalness, and it is questionable if such is conceivable or could be of any practical value; we know that perfect naturalness can only be obtained by the uniform transmission of all frequencies within the music or speech range. It is a matter of fact that it is possible to reproduce any one frequency nearly 10 times (energy ratio) better than another before the ear can detect any departure from naturalness. If we had any measure of æsthetic pleasure, we could obtain a function of considerable value in rating the performance of a loud-speaker by its power to please, simply through an examination of its response characteristic.

The absolute response characteristic of a system may be defined as being a curve plotted between frequency and the efficiency of reproduction at each frequency, this efficiency of reproduction being expressed as the ratio of output to input power. It may thus be greater than unity in the case of an amplifier, but is always less than unity in the case of a transmission line.

It is sometimes more convenient to plot the square root of this ratio, which is then spoken of as the voltage or current ratio. In actual practice it is more convenient to plot relative response instead of absolute response as defined above ; the shape of the curve obtained is similar, the only difference being the scale.

Referring again to Fig. 1, we see that if we cut off all frequencies below 500 cycles the effect on intelligibility will not be appreciable, although the naturalness with its more rigid requirements is noticeably impaired. If we eliminate all frequencies above about 1,700 cycles we reach very nearly the limit of commercial speech, and any further considerable reduction in cut off produces speech difficult to interpret.

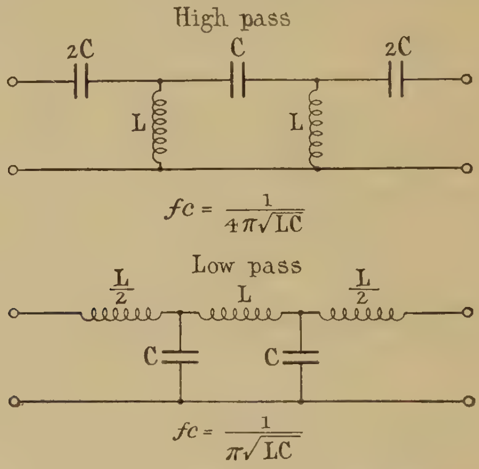

FIG. 2.—CAMPBELL WAVE FILTERS.

The intelligibility is about the same if we cut off all frequencies above 1,500 cycles as it is if we cut off all frequencies below 1,500 cycles.

Fig. 2 shows the form of "high pass" and "low pass" wave filter of the Campbell type suitable for experiments of this kind. The theoretical cut-off frequencies of such filters are given by the following formulæ :—

High pass. $$f_e = \frac{1}{4\pi\sqrt{(LC)}}$$

Low pass. $$f_e = \frac{1}{\pi\sqrt{(LC)}}$$

MEASUREMENTS ON LOUD-SPEAKERS.

We are now in a position to interpret with some degree of discretion the meaning of the response characteristic of a system and the next question that arises is how to obtain such a characteristic. The method of obtaining the response characteristic for an amplifier is well known and there is no need to discuss it here.

What we are concerned with is the response characteristic of a loud-speaker operating in its associated circuit ; it is necessary to lay stress on this owing to the variation with frequency of the impedance of an ordinary loud-speaker.

Dr. A. E. Kennelly has dealt with the subject of impedance measurements on telephone receivers very thoroughly, and has shown that it is possible to relate the shape of the resonant peaks of the response characteristic to the impedance frequency curve of the loud-speaker. It is, however, a rather laborious process and does not give the complete response characteristic. This method is probably very valuable in analysing the cause of inefficiency and distortion, but as a measure of performance a direct measure of the response characteristic is much more informative.

The most obvious way to do this is by means of a calibrated microphone, that is to say, one in which the ratio of output electrical energy to maximum excess pressure due to sound energy is known. It is then only necessary to supply known energy to the loud-speaker and to measure the output sound energy by means of the calibrated microphone in order to obtain a calibration of the loud-speaker.

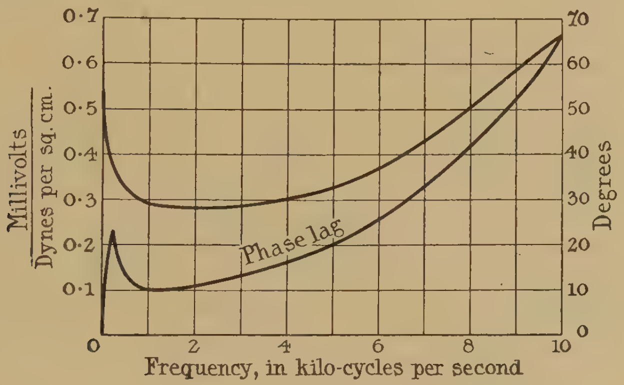

FIG. 3.—ELECTROSTATIC TRANSMITTER.

Fig. 3 shows the calibration of a condenser microphone suitable for the purpose. This calibration is accomplished by comparison with a piston-phone and a thermophone.*

Such a calibration would be absolute ; that is to say, the actual efficiency of transmission between the input to the loud-speaker and the microphone would be known. This is valuable if we wish to measure the power efficiency of the loud-speaker, but if we are only concerned with frequency distortion then we only require to know the relative efficiency of transmission of each frequency.

In theory this sounds extremely simple, but in practice difficulty is experienced owing to the production of standing waves, resulting in a system of nodes and anti-nodes. The result is that while the microphone may be situated at what is a node for one frequency or set of frequencies, it may be also at an anti-node for another frequency or set of frequencies. The result is to produce waves in the response curve ; an obvious but rather laborious method of eliminating the effect of these

* E. C. WENTE : "The Thermophone," Physical Review, Vol. 19, p. 333 (1922) ; also E. C. WENTE : "The Sensitivity and Precision of the Electrostatic Transmitter for Measuring Sound Intensities," *ibid.*, Vol. 19, p. 498 (1922).

nodes is to take a series of characteristics, moving the position of the microphone slightly each time, and then to draw a mean curve.

It is, however, possible by means of suitable damping material on the walls of the room in which the test is made to reduce the effect of nodes and anti-nodes to a degree where they are practically negligible.

Fig. 4 shows a curve obtained by a combination of these two methods, where

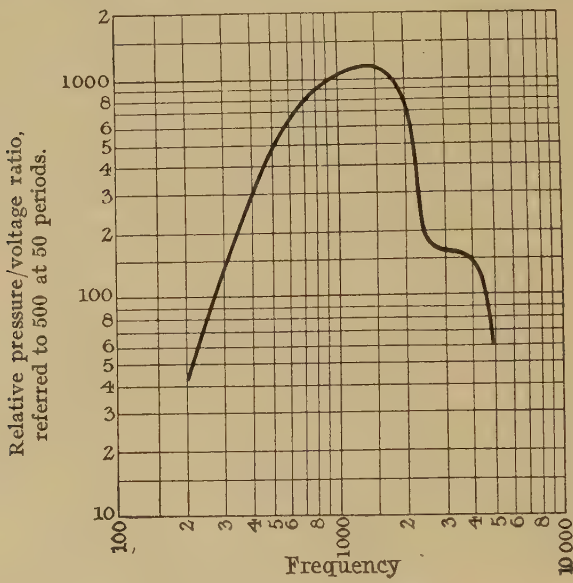

Fig. 4.—Transmission Characteristics of Loud-speaking Telephone Receiver.

the damping was only partial.

The effect of nodes and anti-nodes may be avoided to a large extent by making the test in a large room with the microphone placed very near the horn of the loud-speaker, in which case the volume of emitted energy is very much greater than the volume of reflected energy ; there is, however, a danger of standing waves being formed between the horn and the microphone.

Another method which has been very successfully used by Mr. L. C. Pocock is to employ a band of frequencies, the frequency applied varying sinusoidally in time between two frequencies at equal distances on each side of the frequency, the response of which is to be measured. Such a method automatically takes an average, eliminating the effect of nodes and anti-nodes and actually tending to prevent their formation.

The formation of nodes and anti-nodes may very easily be demonstrated in a room of ordinary size by connecting a valve oscillator to a loud-speaker. It is found that the intensity of sound varies very considerably from point to point in the room, the nodes being roughly of the order of a wave-length apart, so that the frequency of occurrence of nodes is proportionally greater in the case of high frequencies than in the case of low frequencies. The effects may be best observed if one ear is blocked up, in which case it is possible with quite a loud note, if it is sufficiently pure, sounding in a room, to find a position where a single ear can detect practically no sound.

On changing the frequency, an ear in this same position may be found to receive quite a large volume of sound, and the whole system of nodes and anti-nodes will be different.

Returning to the statement that a loud-speaker must be tested in its associated circuit, we can make a modification if we are prepared to make an impedance run on the loud-speaker. In this case we can, if we like, make a test supplying constant current to the loud-speaker at each frequency and then make a correction for its impedance and that of its associated circuit at each frequency. This is not so direct as testing the loud-speaker in its associated circuit, but it is sometimes more convenient. For instance, if we have a loud-speaker operating in the plate circuit of a valve, we can regard the voltage in the circuit comprising the loud-speaker and the plate circuit as being constant at μ times the alternating-current grid voltage, V_g, applied to the valve (assumed to be constant with frequency).

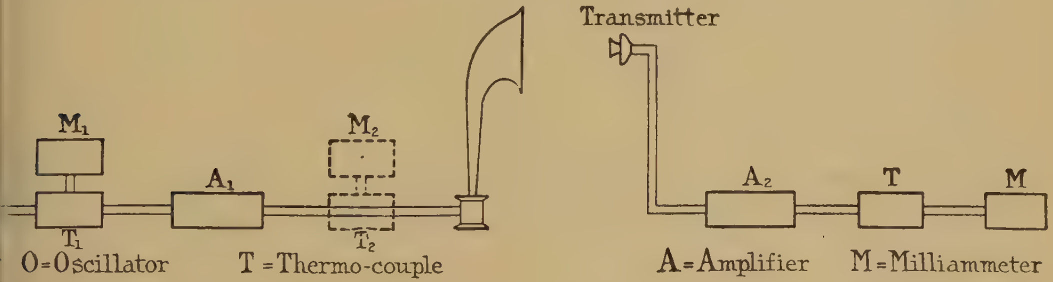

FIG. 5.

Then the current through the loud-speaker, instead of being constant as was the case under test, is given by

$$\frac{\mu V_g}{Z_P+Z_L}$$

where Z_P is the impedance of the plate circuit, and Z_L the impedance of the loud-speaker at any frequency; and the power input to the loud-speaker is

$$\frac{(\mu V_g)^2}{(Z_P+Z_L)^2}\times Z_L \cos\varphi$$

where φ is the angle of the impedance instead of $I^2Z_L \cos\varphi$, where I is a constant (i.e., in the case of a test under constant current).

Hence if the ordinates of our response characteristic are expressed in simple power ratios we must make allowance for the variation in impedance of the circuit by dividing each ordinate by the value of $(Z_P+Z_L)^2$ at each frequency and by multiplying each ordinate by some constant (to give a convenient scale), possibly the value of $(Z_P+Z_L)^2$ at some chosen frequency.

If, instead of power ratios, pressure/voltage ratios are plotted, then we must divide each ordinate by the value of (Z_P+Z_L) at each frequency.

We have described this method because it is the one generally used, but it would appear to be much quicker in the commercial case to test the loud-speaker in circuit with its associated amplifier, since all this correction is thereby avoided and all that is required is the response characteristic for the amplifier, which can generally be made sufficiently flat to be negligible.

Fig. 5 shows the circuit arrangements for a test of this nature. M_1 and T_1 show the position of the meter if the test is to include the amplifier A_1, which must then, of course, be calibrated, while M_2 and T_2 show the position for the constant-current test already described.

In actual practice there may be a large number of variations of this method. One that has been very successfully applied is to arrange that the amplification is such that the net result from the input of amplifier A_1 to the output of amplifier A_2 is a loss at all frequencies. Then it is only necessary at each frequency to insert an amount of standard cable between the oscillator and a headphone to give the same loudness as when the headphone is connected to the output of A_2. It is thus possible to measure the loss in standard miles at each frequency.

Then if P_1 is the input power to amplifier A_1,

P_2 is the output power from amplifier A_2,

$$P_2 = P_1 e^{-2\times 0\cdot 109m}$$

where m is the number of miles of standard cable.

The standard cable used in this case is distortionless and has an attenuation per mile equal to the attenuation of standard cable per mile at 800 cycles per second. By determining the distortion in the rest of the circuit we can calculate that in the loud-speaker.

It has been attempted in this Paper to present the difficulties involved in their most elemental form, rather than to give instances of any very special methods of overcoming them, it being thought that this would be more productive of fresh effort ; and it is hoped that the particulars given in the small part of the subject that it has been possible to cover in the time available will be fruitful in this direction.

THE OVERTONES OF THE DIAPHRAGM OF A TELEPHONE RECEIVER.

By Prof. J. T. MacGregor-Morris, *M.I.E.E.*, and Prof. E. Mallett, *M.Sc.(Eng.)*, *M.I.E.E.*

IT is well known that plates and diaphragms in vibration exhibit resonance at various frequencies, and that at the frequencies of resonance the vibration amplitude under a given impressed force is greater than at others. The frequencies at which resonance occurs, unlike the resonance in the vibration of strings or of columns of air, are not harmonic—i.e., the various overtones are not in the simple proportion of the numbers 1, 2, 3, &c., and the resonance occurs by the plate or diaphragm vibrating in portions and not as a whole. These various modes of vibration can be exhibited by the well-known method of Chladni, simply by sprinkling sand on to a diaphragm. We are much indebted to Mr. Dutton of the City and Guilds (Engineering) College, for assistance in this matter.

The point arises as to what bearing, if any, this has on loud-speakers. Many loud-speakers use a diaphragm or vibrating plate of some kind or other. In some the plate is directly actuated by the electromagnets as in the ordinary telephone receiver, while in others, it is moved by a reed or a moving coil. But whatever the means employed to impart the motion, the facts are the same, that a diaphragm of some form or other is maintained in vibration by an impressed force and so a sound wave is produced.

It seems natural to conclude therefore, that all loud-speakers of these types must have resonant frequencies which are to be ascribed to the various modes of vibration of the diaphragm. The question as to how sharp these resonances are, depends on the damping. In the case of the ordinary receiver considerable damping is introduced by the closeness of the ear cap to the diaphragm, and still further damping is introduced when the receiver is placed to the ear. In at least two different types of relatively successful loud-speakers (*A* and *B*) a similar device is adopted to introduce damping.

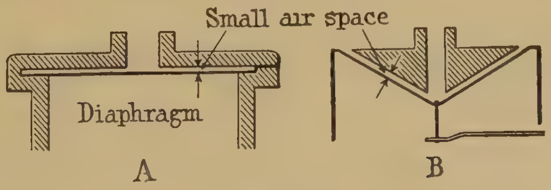

Fig. 1.

It would therefore seem to be a question of a judicious balancing of the damping against the resonance. Without the resonance—i.e., with very strong damping—the response would be very poor, but uniformity over the frequency range would be assured. With resonance the response is great, but there is lack of uniformity.

That the frequencies at which resonance occurs with an ordinary diaphragm are within the necessary range in music seems to be certain, as Table 1* indicates, and as will have been heard during the demonstration.

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* Extracted from a Paper published in the I.E.E. Journal, No. 323, October (1923).

TABLE I.—*Experimental Results obtained with a Standard Receiver.*

Mode of vibration.	Frequency in periods per second.	Ratio to fundamental.
Fundamental	700	1
*1 nodal diameter	1,420 1,630	2·03 2·33
2 diameters	2,250	3·22
1 circle	3,000	4·29
3 diameters	3,400	4·86
*1 circle and 1 diameter	4,000 4,550	5·71 6·50
*1 circle and 2 diameters	5,760 6,150	8.23 8·79
2 circles	6,820	9·74
*2 circles and 1 diameter	8,150 8,600	11·64 12·29

(Those marked * were obtained with one of the receiver coils reversed.)

There are four methods of obtaining the resonant frequencies of a telephone receiver diaphragm :—

(*a*) By intensity of sound produced.
(*b*) By Chladni's sand figures.
(*c*) By Western Electric cathode-ray tube indicating sudden change of power factor.
(*d*) By inductance bridge measurements.

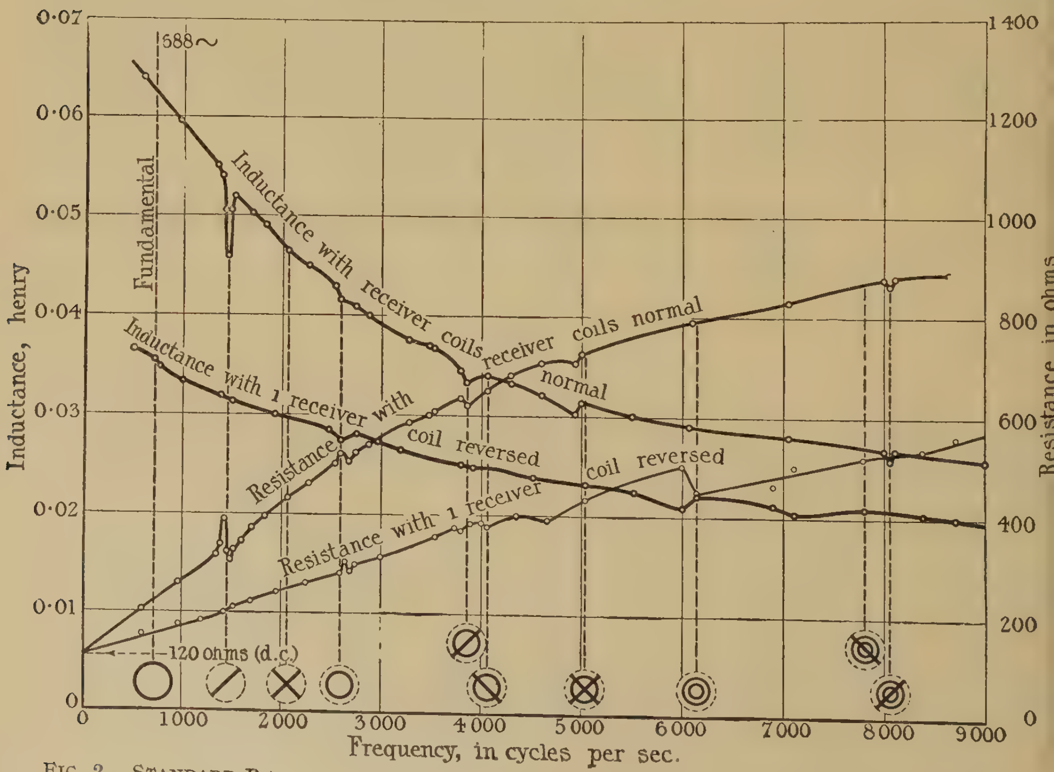

FIG. 2.—STANDARD BELL-TYPE RECEIVER OF 120 OHMS RESISTANCE, TESTED WITH A CURRENT OF 2 MA.

In all cases an alternating current is supplied by a valve oscillator and the frequency is gradually increased, when the various overtones become apparent at their respective frequencies.

In the inductance bridge method the passage through a particular mode of vibration is rendered evident by a sudden drop in the inductance and a return to the smooth curve, and a sympathetic change in the effective resistance curve. These changes have been examined very thoroughly by Dr. Kennelly for the fundamental mode of vibration.

In the accompanying curves (*see* Fig. 2), determined by Mr. M. Stern at East London College, these changes are shown for a number of the overtones as well, and the sand figures belonging to the respective modes are also shown on the graph. Two pairs of curves are given, one with the two coils in the receiver connected in the usual way and the other pair with the two coils in opposition. It will be seen that owing to lack of complete symmetry in construction of the receiver, certain modes are evident in both pairs of curves.

The above results are given in the pitches of the various overtones in Table II., and it will be seen that the plate theory is in close agreement with the observed values ; only in one case is there a difference of more than a semitone.

TABLE II.—*Resonant Points on Standard Receiver* (120 *Ohms*).

Mode.	Coil.	Note produced.	
		Observed.	Plate theory.
Fundamental.*	—	f'' to c'''	f''
╱	Normal	$f'''\sharp +$	$f'''\sharp -$
╳	Reversed... ...	$c''''\sharp$	$d'''' +$
○	Normal	$f''''\sharp$	$f'''' -$
✳	Normal	$g''''\sharp +$	a''''
⌀	Normal	$c''''' +$	$c''''' -$
⊗	Reversed... ...	$f'''''\sharp$	$f''''' +$
◎	Normal	$g'''''\sharp +$	$f'''''\sharp +$
⦸	Reversed... ...	$c'''''' -$	$c'''''' -$

NOTE.—The plus sign means that the note is sharp but by less than half a semitone, and the minus sign that the note is flat by less than half a semitone.

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* Further experiments have shown that the fundamental observed, marked c''' above, is in error owing to masking from a wide band of resonance due to the cap and casing of the receiver, and the actual fundamental is at 686 for the diaphragm. The following values were obtained :—

By inductance bridge without sand : 688·5
,, ,, ,, ,, ,, 684
,, ,, ,, with sand : 668

AUDITORIUM ACOUSTICS AND THE LOUD-SPEAKER.

By G. A. Sutherland, *M.A.*

THE conditions of good hearing in an auditorium are, first, uniform and adequate loudness; secondly, distinctness, which means freedom from undue overlapping of successive sounds; and, thirdly, freedom from distortion, i.e., the preservation of the proper relative intensities in the simultaneous components of a complex sound. A pre-requisite to the attainment of such qualities is the ability to express them in definite form, and also to express with definiteness the way in which the shape and the lining and furnishing of a room contribute to produce these effects.

Uniform loudness is associated in practice with the absence of curved walls, which produce main and subsidiary foci, and, unless suitably covered, always constitute a menace to good acoustics. With flat walls the distribution becomes sensibly uniform within a small interval of time, and, when a source commences to emit sound, the energy density has been shown by Jäger,* making assumptions as to conditions that are sufficiently close approximations to those actually occurring, to grow according to the equation

$$E=\frac{4A}{\alpha vS}\left(1-e^{-\frac{\alpha vS}{4V}t}\right)$$

where E is the average energy density at time t, A is the energy emitted per second, V is the volume of the room, v is the velocity of sound, and e is the exponential. S is the total surface presented by walls, ceiling, floor, furnishings, &c., and α its average absorption coefficient. If there are surfaces S_1, S_2, S_3, S_4, &c., with coefficients α_1, α_2, α_3, α_4, respectively, then clearly

$$\alpha S=\alpha_1 S_1+\alpha_2 S_2+\alpha_3 S_3+\alpha_4 S_4+\ .\ .\ .$$

Similarly, when the source is stopped, the time of decay is given by

$$E=\frac{4A}{\alpha vS}e^{-\frac{\alpha vS}{4V}t}$$

The value of the maximum intensity attained $E_{\text{max.}}$, which is $4A(\alpha vS)$, varies directly with the rate of emission of energy by the source and inversely as the total absorbing power of the room.

If a room be very large, S is necessarily large and the maximum intensity can be changed only by varying A or α. Thus it will be impossible to fashion too large a room satisfactorily for sounds of limited strengths (i.e., where A is small), since to make $E_{\text{max.}}$ large α would then have to be made small, the effect of which is, as will be seen, to increase the rates of growth and decay and so militate against distinctness. The term "adequate loudness" has no meaning except when considered in relation to distinctness. The rates of growth and decay also depend on the value of S/V, which is smaller the larger the room. Thus two rooms having the same proportions and lined with the same material will have different acoustic properties unless they are identical in size.

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* S. Jäger, Sitzungsberichten der Kaiserl. Akad. der Wissenschaften in Wien, Vol. 120 (1911).

The meanings of these expressions for growth and decay can perhaps best be appreciated by reference to the following curves, most of which are due to Eckhardt.* The first series (Fig. 1) shows how the rate of growth in a room of volume 1,000 cubic metres changes with the average absorption coefficient of its walls, the conditions varying from an empty room with bare hard walls to a room with a full audience and a large amount of absorbent lining material. It will be seen that the maximum intensity attained is greatest for the empty room, but that the time to attain this maximum is also greatest. If the decay curves, which are the same curves inverted, are plotted, then the time of decay is clearly seen to be greatest for this case also.

The effect of varying the volume of the room is shown by the next series (Fig. 2).

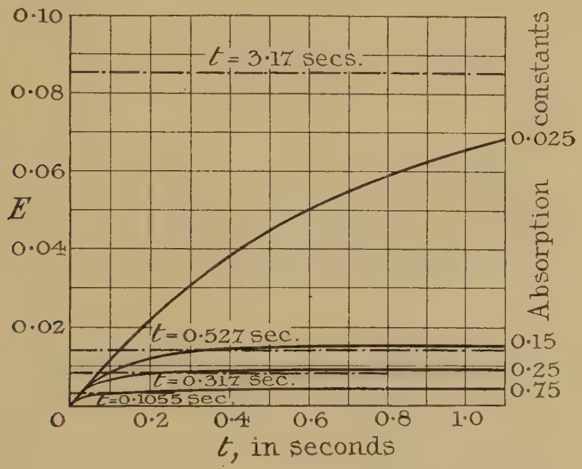

Fig. 1.—Growth of Sound Intensity in a Room for Various Degrees of Absorption of its Boundaries. The Time-Values Associated with the Horizontal Dot-and-Dash Lines give the Time in which the Intensity Grows to 99 per Cent. of the Saturation Value.

The intensity is seen to rise most slowly in the case of the largest room and also to attain the smallest maximum.

In speech, where the emission is discontinuous, the effects may best be demonstrated by drawing a separate curve for every syllable and summing these to show the variation in total intensity. The result is shown in Fig. 3. The dotted curve represents the resultant. In the case selected it will be noted that at any one instant about 60 per cent. of the sound heard is due to previous syllables and only 40 per cent. to the syllable actually being uttered, also that the variation in intensity due to the discontinuous articulation is only a small percentage of the total intensity, conditions which inevitably mean indistinctness.

The effect of speaking at half the rate in the same room is shown in Fig. 4.

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* E. A. Eckhardt, Journal of the Franklin Institute, June (1923).

The maximum intensity is only about half what it was, but the syllable actually being uttered contributes 60 per cent. of this. This represents an improvement, the percentage variation between syllables being 40 per cent. It is to be noted here that, although the discourse seems disjointed to the speaker, it will not seem so to the audience.

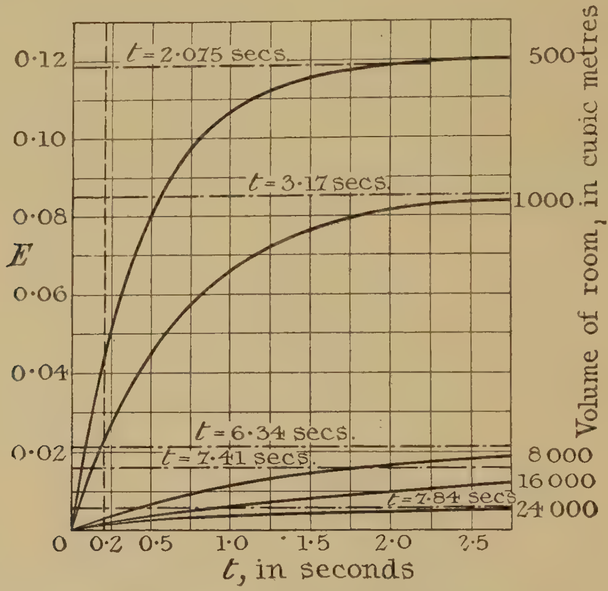

FIG. 2.—GROWTH OF SOUND INTENSITY IN ROOMS OF DIFFERENT VOLUMES, THE BOUNDARIES BEING OF THE SAME POORLY ABSORBING MATERIAL IN ALL CASES.

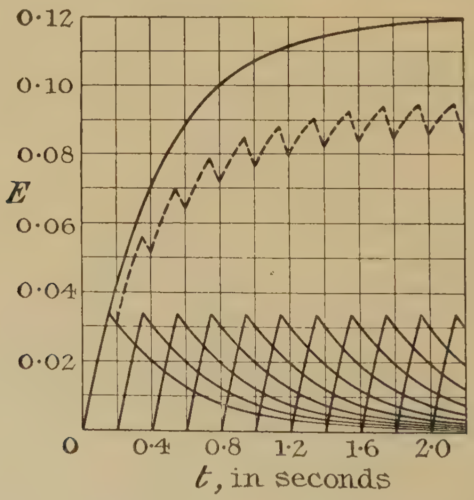

FIG. 3.—GROWTH AND DECAY OF SOUND INTENSITY FOR SYLLABLE EMISSION IN A ROOM OF TOO SMALL ABSORBING POWER.

If we take a room of the same size, but of higher absorbing power, the maximum attained may be reduced to one-tenth of the previous value, as indicated in Fig. 5, but each syllable contributes 94 per cent. of the sound heard at any instant, and the variation between syllables is about 45 per cent. of the maximum. This represents a state of affairs in which hearing is satisfactory, and it will thus be seen that for speech distinctness is more important than loudness.

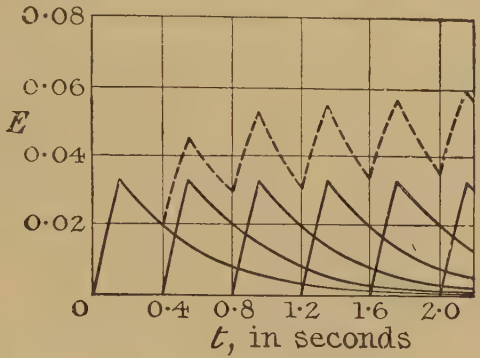

FIG. 4.

It is possible to carry the process further, and in this case we get the effect shown in Fig. 6. The resultant curve differs so little from the components that it has not been drawn. Here the peak intensity is about one-third that of the previous case, but the drop between syllables is about 90 per cent. of this. By common agreement such a state of affairs is assumed to be inferior to the previous state, partly perhaps because of the small peak intensity, partly too because we are so.

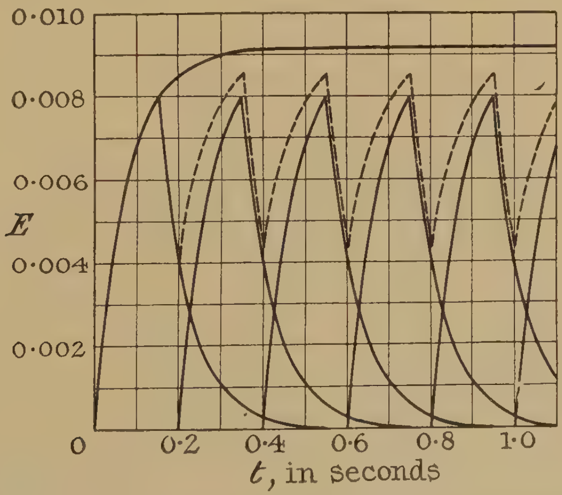

FIG. 5.—GROWTH AND DECAY OF SOUND INTENSITY OF SYLLABLE EMISSION IN A ROOM OF PROPER ABSORBING POWER.

accustomed to a certain amount of overlapping that a condition in which it is absent seems unnatural, but undoubtedly also because ideally a speaker's utterance should have in it something of the character of music, and everyone would unite in condemning as " dead " a passage of music in which there was no blending of successive sounds.

To the slow decay of sound in a room indicated by the foregoing series of curves the name " reverberation " has been given, and exhaustive experiments by Sabine* have shown that the best condition as regards distinctness is associated very approximately with a particular reverberation period which in a room of a given volume has one value for speech, another for chamber music, and a third for orchestral music. A logarithmic curve like these is completely determined if we can measure the time of decay to $1/e$ of the initial value. In any ordinary room this time is too short to be capable of accurate measurement, and Sabine measured instead the time of decay to inaudibility of a sound initially 10^6 times the minimum audible intensity. This

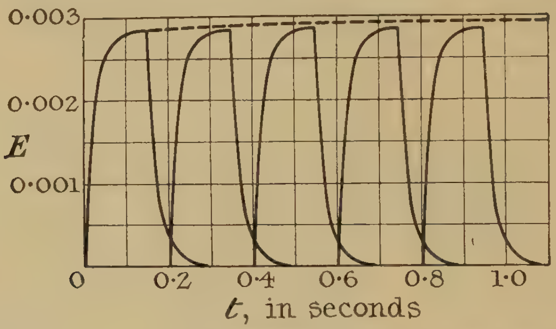

FIG. 6.—GROWTH AND DECAY OF SOUND INTENSITY FOR SYLLABLE EMISSION IN A ROOM OF TOO GREAT ABSORBING POWER.

he calls the " period of reverberation." By doing this in rooms lined with different materials he was able to assign absorption coefficients to linings commonly used. This enables calculation to be made in advance of construction and the desirable reverberation period to be assured from the start. Sound, being a form of energy, cannot be got rid of by the scattering produced by irregularities in the walls, &c. It remains audible until converted either directly by air friction into heat, or into some other form which will eventually become heat. Thus the most effective absorbents of sound are porous linings and furnishings, the relative merits of different linings being indicated by the curves in Fig. 7. Curve 1 is for a painted brick wall; curve 2 is for plaster on tile with a finishing coat; curve 3 is for wood panelling; curve 4 is for a special tile called Akoustolith; curve 5 is for a special hair felt; curve 6 is for hair cushions; curve 7 is for cotton wool cushions; and curve 8 for an audience as ordinarily seated. It will be seen that an audience is the best absorber of sound.

Where speech is concerned the difficulty usually is to introduce sufficient absorbent material, a difficulty which, as we have seen, increases more than proportionately with the size of the room, since the times of growth and decay depend on

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* W. C. Sabine, " Collected Papers on Acoustics " (1922).

S/V, and V increases more rapidly than S. When we come to consider the possible function of the loud-speaker we see that much more satisfactory hearing is likely to be attained by distributing an audience into a number of small rooms with a loud-speaker in each, than by attempting to accommodate all in a large hall. Even if the large hall be full the audience is not so effective in reducing reverberation as in smaller rooms. If the room is partly empty the situation is considerably worse. For music, where a longer reverberation is desirable, the opposite arrangement may give the better results. It is impossible to generalise, however ; each case would have to be settled on its merits.

One thing that stands out quite clearly is that nothing is to be gained by having a loud-speaker with a rate of emission of energy many times that of the human voice. Almost all halls suffer from excessive reverberation, and the louder the initial sound the greater is the confusion produced, since the louder sounds lasts longer and there is more overlapping. This is a matter of common experience ; in fact, a sure indication that a room is suffering from excessive reverberation is furnished if increasing the loudness is found to increase the defect.

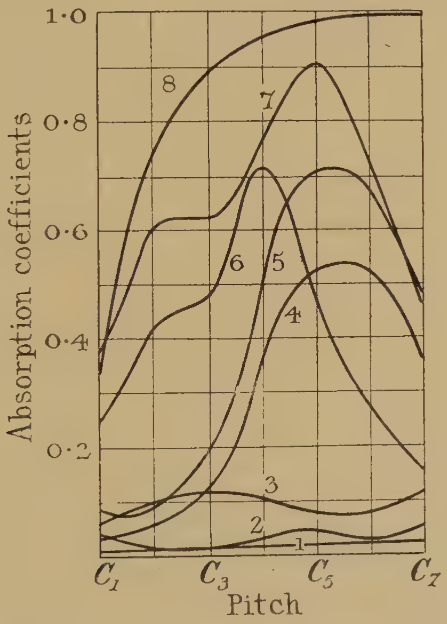

FIG. 7.—SHOWING THE ABSORBING POWERS OF DIFFERENT MATERIALS.

A point to which we have not yet directed our attention, and which can be touched on only briefly, is the question of accurate rendering or freedom from distortion. It is customary for physicists to regard the quality of a note as being completely determined by the relative intensities of the different vibrations leaving the source. Clearly the sound heard depends on the relative intensities of the vibrations reaching the audience. The different wave-lengths, as Fig. 7 shows, are far from being absorbed equally. Sabine quotes a case where with an 8 ft. organ pipe the introduction of felt into an empty room reduced the ratio of the first overtone to the fundamental by 40 per cent., that of the third overtone by 50 per cent., and the fourth by 60 per cent. With a 6 in. pipe, on the other hand, the effect was to accentuate the overtones, whereas all notes below the 6 in. fundamental were purified. The effect of an audience was still different—viz., to purify all notes up to C4, 512, and to have very little effect on tones above this. For C1, 64, the first overtone was decreased 65 per cent. relative to the fundamental, and the second was decreased 75 per cent.

Generally we may say that if the sound from a loud-speaker tends to be too rich in high-pitched notes, then the presence of a large audience is likely to have a corrective effect. As before, it will be advantageous to use several smaller rooms rather than one large one. The case of music is too complicated for treatment here. So far as the author is aware, the judgment of musical authorities has never been expressed in such a form as would make it applicable. No doubt the relative numbers of the different instruments in an orchestra have been fixed empirically, having regard to the conditions normally prevailing in concert halls.

SOME DIRECTIONS OF IMPROVEMENT IN THE LOUD-SPEAKING TELEPHONE.

By S. G. Brown, *F.R.S.*

T is difficult to discuss the problem of the loud-speaker in the short time available, and accordingly my remarks must be confined to one or two general aspects hich occur to me.

It may be remarked that, for the want of the perfect transmission system, is not possible to determine the real inefficiency of the present-day loud-speaker. e presume that it is inefficient because in the electrical transmission to it we have ever approached the perfection of listening direct to, say, the music of the orchestra self.

The loud-speaker consists, among other things, of a diaphragm which operates an enclosed chamber in the manner of the piston of a pump. A hole is provided the cover of this chamber, and air is forced in and out through it as the diaphragm ibrates. The smaller the hole, within reason, the greater will be the velocity of e air as it leaves the chamber, and, as the sound energy is proportional to the uare of the amplitude, the output of the loud-speaker can be materially raised by suitable size of hole.

Connected with this hole is a tapering funnel or trumpet. The object of this to transmit the sound energy to the outer space with as little loss as possible. he air in the funnel vibrates as in a double open-ended tube. The natural period the air enclosed in the trumpet is similar to that in an unstopped organ pipe.

This natural vibration of the air column is super-imposed upon the vibrations t up in the air by the diaphragm. It can, however, be removed by drilling suitable oles in the tube along its axial length. The first hole would be mid-way along e trumpet in order to eliminate the natural frequency of the column. The next vertones are removed by drilling the holes respectively one-quarter and one-eighth the way along the axial length, their diameter being made smaller as the diameter the trumpet diminishes. The improvement brought about by this device is cognisable by a trained ear.

As regards the material of the funnel, a rigid wall is desirable for the proper nclosure of the vibrating air column, and to this end it should be of heavy metal d of thick cross-section. If the trumpet walls are too thin the force of the air panding as sound will distort or swell them, causing them to resonate. (We are eaking, of course, of ideal conditions, and manufacturers, from considerations of eight, etc., must use lighter material.)

The vibrations of the diaphragm are not all imparted to the air, but escape o the metal enclosure and thence up the metal of the trumpet. It is therefore a d plan to insulate the sound-producing element from the trumpet and casing. e manufacturer fits his funnel on by means of a rubber sleeve ; this appears to a good arrangement.

Even the best of modern loud-speakers causes a certain amount of distortion, ch I attribute to the free vibrations of the diaphragm. If we could obtain the necessary air waves with less amplitude of movement at the source, greater freedom from distortion would result. Possibly this might be done by employing a larger diaphragm, suitably stiffened without sacrifice of lightness.

Alternatively, we can produce sound vibrations by other kinds of pressure devices, such as the " Frenophone." This instrument consists of a rotating glass disc and a steel-backed cork pad resting in contact with its surface. The cork is linked to a loud-speaker movement. A telephone receiver presses on the back of the cork by means of an elastic steel pin. Telephone currents, therefore, by operating the receiver, alter the pressure of the cork on the glass disc, thereby varying the frictional drag and working the loud-speaker. It is believed that this combination gives greater clearness in the sound emitted, and the Frenophone may be regarded as a step onwards in the direction of the perfected loud-speaker.

THE CHARACTERISTICS OF A NEW TYPE OF LOUD-SPEAKER.

By Captain P. P. Eckersley.

THERE seems to me to be no doubt that the weakest link in broadcasting at the present moment is the receiver, and that the weakest link in the receiver is the loud-speaker. The British Broadcasting Company is certainly transmitting on an arbitrary basis, but a basis on which remarkably good results can be obtained if the loud-speakers are properly designed. The problem of loud-speakers is balanced between two ideas : Is the aim to be efficiency—i.e., are sounds to be intelligible—or is the transmission to be perfect from the point of view of music ? There appear to be two absolutely distinct types of instrument : the efficient resonant loud-speaker, which will fill rooms of large size, and the loud-speaker which will be just sufficient for an ordinary drawing-room. In the former type I think that much must undoubtedly be sacrificed. It has, however, been pointed out by one speaker how difficult it is to get efficiency and at the same time perfect quality. I propose to demonstrate a loud-speaker of the drawing-room variety which gives very good quality. I feel that it has some qualities that no other loud-speaker has, in that there is a certain warmth in the bass. Many loud-speakers are very resonant at about 1,000 p.p.s. This loud-speaker seems to be resonant somewhat lower in the scale, and I think that this is good from the point of view of music. A really efficient and perfect quality loud-speaker, although it would probably be more expensive and consume a very great amount of power, would be a great achievement. If this meeting helps to bring about that achievement I shall feel that it has not been called in vain. An efficient instrument is possible theoretically, and I think that it will come in time.

[The speaker then demonstrated on the Gaumont-Lumière loud-speaker.]

(*To be continued.*)

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